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THE KARKINOKOSM, OR WORLD OF CRUSTACEA.

By the Rev. THOMAS R. R. STEBBING, M.A., F.R.S., F.L.S.,

Author of "A History of Crustacea," "The Naturalist of Cumbrae," "Report on the Amphipoda collected by H.M.S. 'Challenger,'" etc.

DAN CHAUCER'S well of English undefiled being at the disposal of the naturalist, it is often thought that only out of pedantry or sheer perverseness he fills his story with names and terms borrowed from alien tongues, framing uncouth compounds out of dead Greek and Latin. Instead of saying that the subject now before us is Carcinology (pronounced Karkinology), or the science of Crustacea, it may, therefore, be more acceptable to declare that the discussion will turn on the nature of barnacles, water-fleas, fish-lice, scuds, hoppers, slaters, hodmandods, shrimps, prawns, hermits, lobsters, crayfish, crawfish, and crab-fish. The explanation is not quite so compendious as the word "Crustacea." It is much longer, and yet does not mean so much. It tries to be explicit, and yet remains vague. For, on the one hand, many of the popular names above given are misleading, since no crustaceans are fishes, and some water-fleas and fish-lice are not crustaceans; and, on the other hand, there are several important groups which, because they are seldom seen unless expressly sought for, and because they make no direct appeal to the pleasure or convenience of mankind, have been passed over without receiving any colloquial designation. The truth is that no branch of natural history can be handled with any degree of thoroughness to the exclusion of its own appropriate terms of art; and, as these are intended for cosmopolitan use, there is an advantage in deriving them from the languages of ancient Greece and Rome, which can provoke no international jealousies in the breasts of modern students.

The class Crustacea, omitting one controversial group, may be conveniently divided into three sub-classes called Malacostraca, Entomostraca, Thyrostraca. Of these names the first is primeval, and the second of long standing. Their meanings have ceased to be of importance; it is only the application of them that is important. No one thinks that General Wolfe was especially ferocious, or Charles James Fox exceptionally cunning, or that Bishop Butler had charge of his master's wine-cellar, whatever the circumstances may have been which in the past gave rise to their family names. On the same principle the term Entomostraca (*see* Fig. 1), meaning

FIG. 1.—*Estheria gihoni* (Baird). A Phyllopod of Palestine.

insects with shells, may well be retained, although the animals intended are no longer classed among insects, and many of them are totally devoid of shells. There is a natural craving for descriptive names in science—for names that teach something. That this craving is so seldom gratified is not due to ill nature on the part of the naturalists. Attempts to indulge it are generally failures. The most ingeniously constructed name can scarcely be expected to enshrine more than one striking characteristic of the group it denominates. Now, research is provokingly progressive, and in its progress it is quite fond of showing that the character specified in the ingenious name either does not belong to all the members of the group, or that it belongs also to the members of several

other groups. Thus the intention of the descriptive word is defeated, and, instead of teaching, it leads the unwary learner astray. A name like Malacostraca, signifying soft-shelled, which at one time may have usefully distinguished lobsters and prawns from the oyster and the whelk, is no longer instructive in an enlightened age which could not dream of confusing a tasteful crustacean with a succulent mollusc. Moreover, some Malacostraca have very hard shells, far surpassing in induration those of the Entomostraca and of many Mollusca. The name Thyrostraca, meaning shells with doors or valve-shells, gives a small item of information about cirripedes, while the latter more familiar name refers to the fact that the cirri or legs of a barnacle have some resemblance to ringlets or tresses of hair (*see* Fig. 2). None the less, some of the group have no shells and no valves and no cirri.

FIG. 2.—*Lepas anatifera* (Linnæus). A pedunculated Cirripede.

In this opening chapter it would be highly proper and methodical to define the class under discussion in such a way that any schoolboy, or a poet, or a journalist, on coming casually across a *Notopterophorus papilio*, for example, might, under the guidance of the definition, be able at once to exclaim, "Lo! here is a crustacean!" But nature, rejoicing in the penumbra and the twilight, and abhorrent of every hard line, takes a pleasure in setting definitions at defiance, varying the characters within a group, and adding here and subtracting there, till there is pretty well nothing left which all the confederated members can claim to have in common. What if some of the Crustacea are endowed with a crustaceous integument; with gills for breathing; with a heart; with eyes and brain; with segmented body and limbs; with bilateral symmetry and with powers of locomotion? There are others which are soft-skinned, without gills, eyeless, brainless, heartless, shapeless creatures, in a state of fixation (*see* Fig. 3). The difficulty of defining natural groups may be illustrated in this way. Suppose that three sets of animals have characters so combined that they may be represented respectively by the letters *ab*, *bc*, *ca*, or by the colours red and yellow, yellow and green, green and red. The symbols indicate that each set has half its characters in common with each of the other sets. Yet there are no characters common to all three sets, so as to be available for defining a higher group embracing them all. When in such circumstances a definition has to resort to negative and alternative characters, it may be logically exact, but it loses the quality of helpfulness. The beginner, therefore—perhaps the resentful beginner—must say what he pleases, and make what he can of the statement that the division of the Arthropoda called Crustacea have a segmented body and limbs at some stage of life; that either they have gills or else they breathe in water through their skin; that they have no proper neck; that they never have wings; and that they are born in locomotive freedom. Like insects, they have an integument composed of a substance called chitine. This may be extremely flexible, or, passing through various degrees of tough and brittle, may, by the copious addition of chalky material, attain the hardness of bone or brick.

Having come to a provisional agreement with ourselves that an almost indefinable congress of startlingly incongruous-looking creatures are all to be admitted to the honourable title of crustaceans, we are next tempted to ask what natural bond of union, if any, exists for such an assemblage. Were they all separately invented just as we find them, with their striking contrasts and innumerable gradations and subtle resemblances; or, have they been evolved in ramifying lines from a common root? The first hypothesis would leave us rather idiotically gaping at what must seem to be the effects of an unfathomable caprice. Probably, therefore, most thinking men would now prefer to explain the genesis of the "Karkinokosm," as we know it, on the principle of evolution. By this we mean that all the forms, now so amazingly unlike one another, are nevertheless descended from common ancestors. No one denies that animals are capable of reproducing their kind. No one denies that children are more or less unlike their parents and unlike one another. That these unlikenesses can be to some extent accumulated has been proved. That in the course of nature they are capable of an accumulation so extended and so permanent as to separate a man from a mouse, or the great *Cardisoma guanhumi*, figured on the next page, from the worm-like parasite *Lernæolophus sultana*, is yet awaiting proof. To the principle of evolution it matters not how the variations are produced, so long as some of them can sometimes be secured against reversion to the ancestral pattern. So far as the principle is concerned, it is indifferent whether the changes result in exalting or degrading the character of a species. To explain the existing constitution of the class Crustacea, it must be supposed that some of its members have risen, and that some have, after rising, fallen. If it cannot be proved that all have been evolved from a common stock, something can be said for the probability of it; and those who are dissatisfied can only be asked to provide some other explanation that will better fit the phenomena.

FIG. 3.—*Lernæolophus sultana* (Nordmann). A Copepod, parasitic on Fish.

For the purposes of a natural classification it is the history of evolution that is most wanted. We need to trace back the ancestry of different forms to the point of junction, just as we follow the twigs of a tree to the branch from which they spring, and the branches to the common stem. Clearly this can only be done by help of the palæontologists. What the rocks have as yet revealed as to the succession in time of crustacean forms has recently been represented by Dr. Henry Woodward* in a kind of fossil tree. Of the undisputed Crustacea he recognizes eleven principal branches, and all these he draws as running parallel down to the Carboniferous period—a period so ancient that in calculating its age imagination and arithmetic have to play a drawn game, and yet so modern that in it the merry cockroach is already in evidence. The disappointing inference is that any

* *See* his Presidential Addresses to the Geological Society of London, 1895, 1896.

common starting point for all the Crustacea must lie indefinitely further back; and in fact it is not till the pre-Cambrian period that all the branches are made to join the central stem, while of the earlier points of junction between the branches themselves it must be admitted that

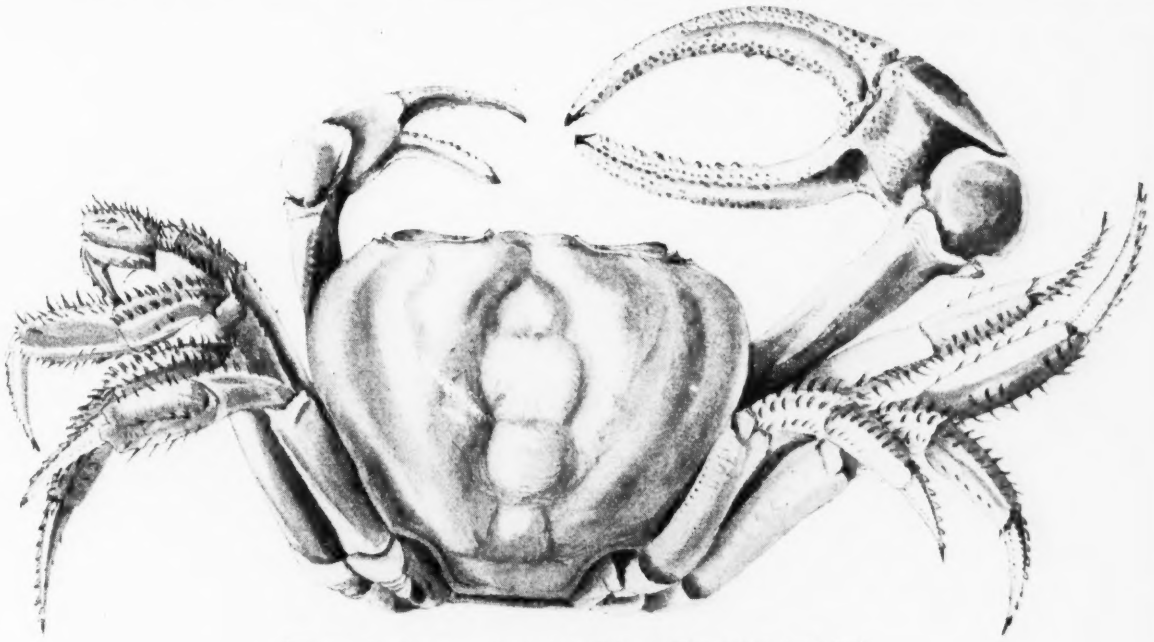


FIG. 4.—*Cardisoma guanhumi* (Latreille). A West Indian Land Crab.

most are highly conjectural. The true affinities of a modern species are often only discovered by careful dissection, and such a process is rarely possible with mangled remains in an obdurate fossil. Sometimes, when the rock specimens are exceptionally clear, the characters displayed are distressingly like those familiar to us in living forms. Thus, according to Dr. Ortmann, a fossil crawfish from the Upper Chalk is more nearly related than any extant species to the modern *Linuparis trigonus* (De Haan) of Japanese waters. It is ungracious to find fault with nature. Perhaps the researches of geology are in fault, or perhaps there are rays, yet waiting to be discovered by the physicist, which will penetrate the secrets of an obliterated past. Properly to attest the work of evolution in nature we sorely need to recover a series of lost pictures. They should be a kind of dissolving views carrying us back to the dawn of life, with the features of all existing forms not abruptly but gradually fading away, till they melt into an undifferentiated original. Some generalized forms are indeed quoted from the record of the rocks, but they are few and obscure compared with the desires and expectations of the evolutionist.

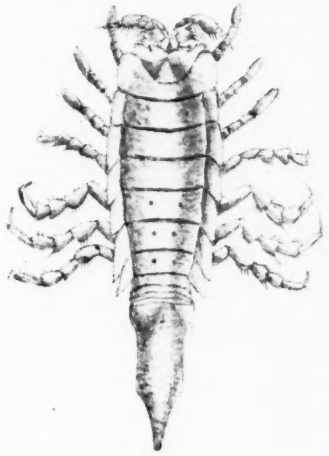


FIG. 5.—*Glyptonotus sabini* (Kröyer). An Arctic Isopod.

In a future chapter an attempt will be made to show how a belief in the unity of the class Crustacea may be founded on the internal evidence of extant species. That this is not, on the face of it, a very simple task, might be inferred from the few illustrations here brought together. They represent a decapod, an isopod, a phyllopod, a parasitic copepod, and a cirripede or cirrhopod, thus ranging from the highest to the lowest ranks of the crustacean commonwealth. Since nature has ordained that the writer of "Hamlet" should have personal identity in common with a speechless babe, a land crab need not be too proud to own a barnacle for its distant cousin.

A DROWNED CONTINENT.

By R. LYDEKKER, B.A., F.R.S.

AS many of our readers are doubtless aware, deep boring operations are being undertaken in the island of Funafuti, in the Ellice group of Polynesia, with the primary object of ascertaining the depth to which coral rock, or limestone of coral origin, extends. If it were found that such coral-made material extended to depths far below the level at which living coral can exist, there would be evidence that the island on which the experiment was conducted had subsided. And if subsidence were thus proved to have taken place in a single island selected almost at random, the conclusion could hardly be resisted that the greater part, if not the whole, of Polynesia must likewise be a subsiding area, or, in other words, the remnants of a drowned continent, some of the higher lands of which are indicated by the atolls and other islands of the Coral Sea. It is, therefore, a favourable opportunity for a few

words in regard to the permanence or otherwise of the great oceanic basins and continental areas of the globe. This subject, it need scarcely be said, has not only an intense and absorbing interest of its own—for it is difficult for anyone except a geologist to fully realize that the solid ground on which he stands may have been buried fathoms deep beneath the water—but is also one of the utmost importance in regard to many puzzling problems connected with the present and past geographical distribution of terrestrial animals and plants on the surface of the globe.

Although it might well have been thought that opinion in matters scientific would be unlikely to veer suddenly round, and after tending strongly in one direction incline with equal force in the one immediately opposite, yet there are few instances where the swing of the pendulum of opinion to one side has been more swiftly followed by its oscillation to the other than has been the case in the problem of the permanency of continents and oceans. When geology first began to take rank among the exact sciences, and it was demonstrated that most of the shells and other fossils found in the solid rock of many of our continents and islands were of marine origin, it was a natural, if hasty, conclusion that land and sea had been perpetually changing places, and that what is now the centre of a continent might comparatively recently have been an ocean abyss. Accordingly, when any difficulty in finding an adequate explanation in regard to the geographical distribution of the animals or plants of two or more continents or islands occurred, the aid of an "Atlantis" or a "Lemuria" was at once invoked without misgiving, and a path thus indicated across which the inhabitants of one isolated area could easily have passed to another.

This was one swing of the pendulum. But as the methods of geological observation and investigation became more exact and critical, it was soon obvious that, in many areas at least, the alternations between sea and land could not have been so frequent or so general as had been at first supposed. It was, indeed, perfectly true that many portions of some of our present continents had for long periods been submerged, or had been at intervals alternately land and sea. But at the same time it began to be realized that the fossiliferous marine deposits commonly met with on continents and large islands were not of such a nature that they could have been laid down in depths at all comparable to those now existing in certain parts of the basin of the Atlantic. Even a formation like our English chalk, which had been supposed to have analogies with the modern Atlantic deposits, appears to have been laid down in a sea of much less depth and extent, and probably more nearly comparable with the modern Mediterranean. Then, again, it was found that large tracts in some of our present continents, such as Africa and India, had existed as dry land throughout a very considerable portion of geological time. Moreover, it was asserted that no formations exactly comparable to those now in course of deposition in the ocean abysses could be detected in any of our existing continents or islands; while it was further urged that in none of the so-called oceanic islands (that is, those rising from great depths at long distances from the continental areas) were there either fossiliferous or metamorphic rocks similar to those of the continents and larger continental islands.

This was the second swing of the pendulum, and for a long period it was confidently asserted that where continents now exist there had never been any excessive depth of ocean; and, conversely, that in the areas now occupied by the great ocean abysses there had never been land during any of the later geological epochs. It was, indeed, practically affirmed that wherever the sounding-line indicates a thousand fathoms or more of water, there sea had been practically always, and that no part of the present continents had ever been submerged to anything like that depth.

Almost as soon as the pendulum of opinion had attained the full limits of its swing in this direction (and this swing had been largely due to the influence of geologists and physicists), there began to be signs of its return to a less extreme position. It was, in the first place, proved that a few deposits—and these of comparatively recent date—analogous to those of the ocean abysses, do occur in certain areas. And, in the second place, it was shown that a few oceanic islands do contain rocks like those of the continents, and are not solely of volcanic or organic origin. Zoological and palæontological discoveries were at the same time making rapid advances; and the students of these branches of science, who had been among the foremost in giving the swing of the pendulum on the side of continental instability its first impulse, now began to press their views—only in a more moderate manner—in the same direction. Evidence had long been accumulating as to the identity of certain freshwater formations and their included animal and plant remains occurring in South America, South Africa, India, and Australia; and it was urged that during the Secondary period of geological history not only was Africa connected with India by way of Madagascar and the Seychelles, but that land extended across what is now the South Atlantic to connect the Cape with South America, and that probably India was likewise joined to Australia by way of the Malay archipelago and islands. In fact, there seems good evidence to indicate that at this early epoch there was a land girdle in comparatively low latitudes encircling some three-fourths of the earth's circumference from Peru to New Zealand and Fiji.

Even taking into account the comparatively early date of its existence, this girdle of land, the evidence in favour of which can scarcely be shaken, gave a heavy blow to the adherents of the absolute permanency of continents and oceans, as it clearly indicates the comparatively modern origin of the basin of the South Atlantic. But this is not all. South America, which there is good evidence to believe was long cut off from the northern half of the New World, shows certains indications of affinity in its fauna with that of Europe in early Tertiary times, and to a certain extent with that of modern Africa; and the only satisfactory way of explaining these relationships is by assuming either the persistence of the land connection between the Cape and South America across the South Atlantic till a comparatively late geological epoch, or that such connection took place further south by means of the Antarctic continent. There are several objections, which need not be considered here, in regard to the latter alternative; and since there is other evidence in favour of the comparatively recent origin of the South Atlantic depression, the persistence of a land connection in lower latitudes seems the more probable explanation.

In addition to all this, there is evidence of a more or less intimate relationship between the land faunas of Australasia and South America; and as similar types are not met with in Africa, and several of them belong to groups unlikely to have endured Antarctic cold, it has been suggested that America and Australasia were in connection at no very remote epoch by way of the Coral Sea. It is known, for instance, that some of the Australian marsupials are more or less closely allied to others which inhabited South America before it was connected with North America; and as no kindred types are met with

either in the latter area, in Europe, or in Africa, a land connection by way of the South Pacific, and that at a comparatively recent epoch, offers almost the only satisfactory explanation of the means of transit, if the Antarctic theory be rejected. And it may be mentioned in passing that the acceptance of even the latter would imply a large modification from the existing distribution of land and water in the southern hemisphere.

But the evidence for a land connection by way of the Pacific does not by any means rest on the testimony of marsupials alone. Passing over certain groups, it may be mentioned that the earthworms of Australia and New Zealand are strangely like those of Patagonia, and have no very near relatives in Africa; while an almost equally strong affinity is stated to exist between the Patagonian and Polynesian land slugs. Neither of these groups of animals are fitted to withstand the cold of high latitudes, and it is difficult to see how the members of the second, at any rate, could have reached the two areas by any other means than a direct land connection.

Turning now to the brief reports hitherto received as to the results of the Funafuti boring, it appears that this has been carried far below the limits of coral life, and is still in coral limestone. So far, therefore, the advocates of the theory that Polynesia is the remains of a sunken continent have scored a great triumph; and although there is still the possibility that some of the atolls in this vast area may prove to be perched on the denuded summits of extinct submarine volcanoes, even this would not interfere with the general conclusion. If deeper borings should result in touching rocks more or less similar to ordinary continental sedimentary deposits or metamorphic crystallines, an even firmer basis would be afforded to the hypothesis of subsidence which has now received such striking confirmation.

As the result of the boring it appears, then, that there is a possibility that the community between the South American and Australasian faunas may admit of being explained by means of a direct land connection between the two areas at a comparatively recent geological date. Even, however, if this explanation receive future support and acceptation, there are, as in all similar cases, still many difficulties with which to contend. One of these is the practical absence of all non-volant mammals from Polynesia, with the exception of the Solomon group, where a few cuscuses and rats are found. But the case of the West Indies—where there is every probability that there was formerly a large mammalian fauna, the majority of which were drowned by submergence—may very likely afford the solution of the difficulty. Worms and slugs would probably find means of survival in circumstances where mammalian life would disappear. This explanation will, however, clearly not apply in the case of New Zealand, where, if mammals had ever existed, their remains would almost certainly have been discovered. It must be assumed then that, if Polynesia was the route by which the faunas of Australia and Patagonia were formerly connected, New Zealand was at that time isolated. And, indeed, seeing that the hypothetical land connection between the areas in question must have existed at a comparatively late epoch, it is most likely that the ancient Polynesian land was already broken up to a considerable extent into islands and archipelagos, so that the main line of connection may have been but narrow, and from time to time interrupted. Indeed, it must almost of necessity have been but incomplete and of short duration after the introduction of modern forms of life, as otherwise the types common to Australia and Patagonia would be much more numerous than we find to be the case. Hence there is no improbability in the suggested isolation of New Zealand during the period in question.

But, putting these interesting speculations aside, the results of the Funafuti boring indicate almost without doubt that Polynesia is an area of comparatively recent subsidence; and it has already been mentioned that there are good reasons for regarding a large part of the basin of the South Atlantic as of no great antiquity, while the area of the Indian Ocean appears to have been considerably enlarged during the later geological epochs. Apparently, therefore, the great extent of ocean at present characteristic of the southern hemisphere is a relatively modern feature.

Hence it is clear that the extreme views prevalent a few years ago as to the absolute permanency of the existing continental and oceanic areas clearly stand in need of some degree of modification. And what we have now to avoid is that the pendulum should not once more take too long a swing in the opposite direction.

So far as the great continental masses of the northern hemisphere are concerned, it would appear that portions of these have always existed to a greater or lesser extent as land. But the great extent and homogeneous character of formations like the Mountain Limestone, the Chalk, and the Nummulitic Limestone, suggest that sea was much more prevalent in this area than it is at present, and that, so far as the Old World is concerned, the continental area has been growing. The North Atlantic, and probably also the North Pacific, may apparently be regarded as basins of great antiquity. On the other hand, in the southern hemisphere, although Africa, parts of Australia, and at least some portions of South America, are evidently land surfaces of great antiquity, they, together with the islands of the Coral Sea, seem to be mere remnants of a much more extensive southern continent or continents. Conversely the southern oceans have gained in area by swallowing up these long-lost lands. Obviously, then, although true in a degree, continental permanency has by no means been the only factor in the evolution of the present surface of the globe.

IS WEATHER AFFECTED BY THE MOON?

By ALEX. B. MACDOWALL, M.A.

THE history of science, in its relation to popular beliefs, often affords on both sides curious illustrations of the old adage, *Humanum est errare*. Certain ideas as to the causation of natural phenomena are widely prevalent. Science steps in to examine them. She tests and measures; sees them to be very faulty; puts them aside as worthless and vain. But there comes a time when this judgment has to be revised, and considerable grains of truth are found among the rubbish.

There are at present signs, if I mistake not, that the denial of lunar influence on weather has been made too confidently.

If we ask any working gardener, or fisherman, or sailor, whether he thinks the moon has anything to do with weather, he will probably reply with a ready affirmative. He may enlarge, in his own wise way, on what weather we have to expect if the change of the moon is at this hour or that; if the moon is high or low; if the new moon is on her back or standing up, and so on. Popular weather lore on this subject is, we all know, plentiful; and in reading a collection of those sayings we are not exactly impressed with their harmony or consistency. The pages of Aratus, of Virgil, of Bacon, witness to the venerable character of this class of "saws." And the

North American Indian of to-day considers the position of the moon's horns with the same practical interest as the Scottish peasant.

Over all this, it would appear, science shakes her head doubtfully. Lunar influence may be probable, but it is not proven. Some would even go further. Let us listen to a few authoritative utterances on this point.

In 1895 I find the head of the United States weather service remarking that "Lunar periods [in weather] . . . have all failed to get a foothold in scientific respect, though much time has been put upon them, and they appear theoretically probable."

Prof. W. Morris Davis, author of one of the best recent books on meteorology, says: "The control of the weather by the moon has long been a favourite idea, but it has not been found to bear the test of accurate comparisons of weather and lunar phases, except in a very faint and imperfect manner."

Once more, Sir Robert Ball, in his "Story of the Heavens," says: "Careful comparison between the state of the weather and phases of the moon has quite discredited the notion that any connection of the kind really exists."

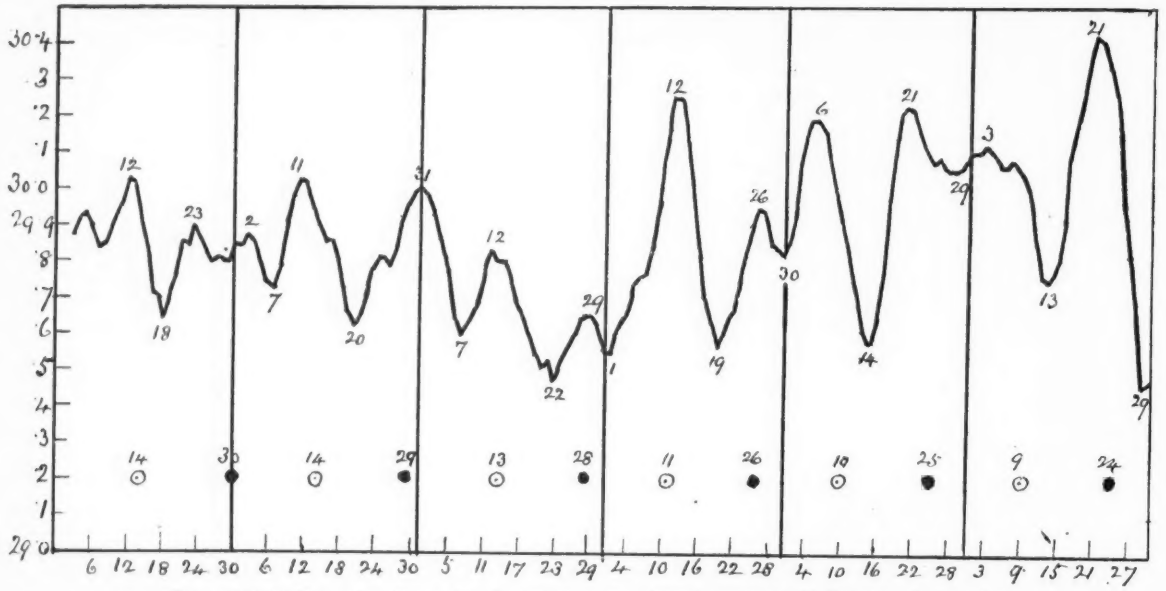
Nevertheless, further study is being given, and will doubtless continue to be given, to this interesting question. Of recent work upon it, may be mentioned that by M. Garrigou-Lagrange, described in a series of papers to the Paris Academy. He attributes to the moon's influence certain periodical oscillations of the pressure and gradients between the Pole and the Equator observed in the meridian of Paris. These are superposed on others which he considers due to the sun; and the effect is different according as the moon is in a northerly or southerly position.

A simple and direct way of seeking light on the subject of lunar influence is to plot a number of curves of daily barometric pressure, and see whether any extensive correspondence with the moon's phases can be made out. Having recently done this with the Greenwich data, I would invite attention to some facts which appear to me to be highly suggestive. We seem to find this state of things: a certain definite relation to the moon's phases (speaking roughly, a barometric wave to each new and each full moon) appears from time to time, and persists, perhaps half a year, or more. Then it may disappear (from some cause or other), to reappear later on.

The half-year closing with November, 1897, is, it so happens, a very good example. In the accompanying diagram the curve is that of the daily barometer at Greenwich from June to November, smoothed with averages of five; that is, each day point of the curve represents the average of five daily values (*e.g.*, that of the 3rd of June, the five days, one to five, and so on).

This curve presents, it will be seen, a series of waves corresponding remarkably with the moon's phases. Are we prepared to affirm that so many coincidences are merely fortuitous?

This correspondence still persists at the date of writing (December 7th), and readers of KNOWLEDGE may be interested to watch further developments. Doubtless, it will be masked or obscured ere long; and it may, of course, be argued that those intervening periods of irregularity (or, in some cases, a different kind of regularity) suffice to overthrow the evidence of casual connection in periods like that here considered.



Curve of Daily Barometer, Greenwich, June to November, 1897 (smoothed with Five-Day Averages).

Going back as far as 1879, curves of the same type as that here given, and of similar extent, will be found in 1883, 1884, 1889, 1893, and 1894. Why the correspondence should come out more clearly at these dates I am unable to say. Perhaps some astronomical cause can be assigned.

It is easy to see how an experience of long and regular recurrences in weather like that of the years indicated may have given rise to a popular conviction that the moon influences weather; and, on the other hand, the fact of irregularity subsisting and alternating with regularity might account for the negative results often arrived at by meteorologists when they have superposed the weather data for a long series of lunations.

The presumption of continuance in the type of weather indicated, which the above facts appear to warrant, in a measure might afford some useful help in forecasting.

SERPENTS AND HOW TO RECOGNIZE THEM.

By LIONEL JERVIS.

THE casual visitor to the Zoological Gardens should have little difficulty as a rule in identifying a snake. The name is written underneath in Greek or Latin, or half in Greek and half in Latin, or in a latinization of local names, as, for instance, in the case of the hamadryad, *Naja bungarus; Naja* being, I take it, an adaptation of "nâg," which is the Hindî for cobra, and *bungarus*, I suppose, originates in the *bungarum* of Russell's "Indian Serpents."

For all that, the scientific names are better than the local ones. Take, for example, the *Lachesis lanceolatus*. Perhaps *Bothrops* or *Trigonocephalus* are more familiar titles than the comparatively recent *Lachesis*, but everyone knows what the *Lachesis*, *Bothrops*, or *Trigonocephalus lanceolatus* is; it is, of course, the *fer-de-lance*. Combining the nearly related *Lachesis atrox* (the difference between the species is so slight that even specialists are unable to differentiate offhand), let us see how many local names we can find. First there is the *fer-de-lance*, then follow the rat-tailed pit-viper, the lance-headed viper, the deadly snake ("deadly" is a "very vile" prefix, quite unworthy of the Zoological Society, who, if I am not mistaken, were guilty of it), the jararaca, the yellow viper, the whip snake, the Labarri snake, and I dare say that there are half a dozen other names in Tropical America for this serpent. Nevertheless it is, I think, better to leave the local English name alone than to invent one. Look at the shielded death adder (*Notechis scutatus*), till recently known as the short-death adder (*Hoplocephalus curtus*). The colonists call it, very happily, the tiger or brown-banded snake—a look at the serpent will show you why. But here we have "death adder." Why "death adder"? The death adder of the colonists, the "unqualified" death adder of Regent's Park—the *Acanthopis antarcticus*—is about as unlike a tiger snake as it well can be. The tiger snake has a cylindrical body, tapering into a respectably proportionate tail; the body of the death adder is bloated, and terminates in a short compressed tail with a spike at the end of it. In both the head is distinct from the neck: that of the tiger snake, which resembles a cobra's, very slightly; that of the death adder, which resembles a viper's, very markedly. The prefix "shielded" is good enough, but "short" is not so happy, considering that the *Notechis* is about twice the length of the *Acanthopis*. Again, why "purplish death adder" instead of "black snake"? Why not confine "death adder" to the *Acanthopis* instead of applying it aimlessly to almost every poisonous snake in Australia?

Just one more warning as to the danger of trusting to the accuracy or sense of either the English or scientific title. Everyone has heard of the beautiful and venomous coral snake of Tropical America. It is very brilliantly marked with rings of black and red, with thin whitish edges to the black rings, and from this the Spanish-speaking inhabitants very happily named it the "corràl" or "ringed" snake. Some naturalistic genius gets hold of this, and, forcing the local name into Latin, calls it *Elaps corallinus*, thereby misleading people into the idea that it is a bright red snake, and called *corallinus* from its resemblance to coral.

Nevertheless the descriptive label furnishes the accepted name, such as it is; but labels are very little use when there are two or three different species in one case, as snakes cannot be expected to remain opposite their respective descriptions, any more than monkeys. How, then, are we to identify them? Coloration is not always a sure guide. Look once again at the *Lachesis lanceolatus*. It may be of one uniform colour above—grey, brown, yellow, reddish, or olive, or it may be any of these colours with regular or irregular dark markings, or almost any combination of the foregoing.

In a short article, or even in a small pamphlet, it would be impossible to give a "ready recognizer," even for snakes which are easily distinguished by specialists. I shall therefore content myself with giving an object lesson from the small genus *Ancistrodon*,* of the sub-family *Crotalinæ*, or pit-vipers.

The first distinguishing feature is the pit in the loreal region, between the eyes and the nose. This pit characterises a group of poisonous snakes the bite of which is sure to entail very unpleasant, often fatal, consequences. This group falls into two main divisions—those which have rattles on their tails and those which have not. Of course a snake with a rattle on its tail is a rattlesnake, and when you see a serpent of this kind you are quite safe in saying: "That is a poisonous American serpent"; and if it is described as *Crotalus terrificus*, you may, if you like to take a slight risk, add: "That is the only one of the kind found south of Mexico." But there is a pitfall here, as you are quite likely to find the South American rattler described as "*molossus*" or "*horridus*," and "*terrificus*" has been applied to the water-rattle. Oh for an universal classification!

The pit-vipers which have no rattle, again, fall into two subdivisions: those in which the head is covered with scales, the *Lachesis*, and those in which the head is shielded by nine symmetrical plates, the *Ancistrodon*. So, then, if you find a serpent with a pit between the eyes and the nose (not with two or more pits in the upper lip shields), with the head covered with nine shield-like plates, and with no rattle on its tail, you have an *Ancistrodon*.

Of course the most satisfactory state in which to examine a poisonous snake is when it is dead and pickled. If alive, the best thing to do is to secure its neck in a snake-loop. Failing this, you must content yourself with looking at it through glass or wire or from a safe distance. The first part of the following descriptions will, therefore, apply when the snake can be examined minutely; the second when it can be seen only under comparative difficulties. In some cases the colour pattern, together with the data already given as to the pit, etc., will be sufficient to identify the serpent.

First I select the only species of the subdivision in which the second upper labial forms part of the border of the loreal pit and the sub-caudal shields are in pairs.

A. hypnale.†—By these features you cannot fail to recognize the snake on close examination; otherwise it is not so easy to identify. It is only a little bit of a snake, with a turned-up nose. The colour is generally dark—usually greyish or brown—with or without spots, and the markings on the head are ill-defined. The internasals and præfrontals, it is true, are broken up into scales, but you usually require a magnifying glass to make this out. The local name on the west coast of India and in Ceylon, where it is found, is the "carawila." As far as I know, there is but little danger attending the bite.

A. piscivorus.‡—The first of four species in which the second upper labial forms part of the border of the pit, and some of the sub-caudals are single, some in pairs.

Remarkable for being the only one of the *Ancistrodon* in which the loreal is absent. The sub-caudals are sometimes

* Greek: "fish-hook toothed." † Greek: "sending to sleep."
‡ Latin: "fish-eating."

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all single; the third upper labial is very large, and usually enters the eye.

You are at once struck by the bluntness of the muzzle and the closeness of the eye to the nose, which gives it a very vicious appearance. The colour is generally sombre—usually a dark slaty brown—with darker cross markings. It is said to be of a quarrelsome disposition (some former naturalists qualified it as *pugnax*), and it will certainly fight with anything that is put into the same case, be it rat, snake, or stick. For all that, I have heard that it is easily tamed, and becomes, for a snake, quite affectionate. It attains a length of about four feet, and is very heavy and bulky for its size. It is plentiful in the south-eastern United States, where, under the names of "cottonmouth," "water mocassin," and "water viper," it enjoys a very bad reputation, which was confirmed in my mind by a sad story I heard the other day from a trustworthy source. A certain good sportsman, while fishing in Florida, had made his camp near the water; and finding, after supper, that he had left something in the boat, he desired his servant to go and fetch it. The servant, who was native to the country, hesitated, saying that he heard a mocassin out fishing; but his master, after listening for some time and hearing nothing, pooh-pooh'd the idea. The poor fellow reluctantly obeyed, and on his way to the boat was struck, and died in a few hours. I gathered from my informant that these serpents are quick to resent any trespass on their riparian rights, especially at night.

Second, *A. bilineatus*.*—Easily to be identified by the markings on the head. Round the canthus (the sharp upper edge of the snout) is a fine yellow line, which usually broadens out as it passes behind the eye to its termination on the neck: immediately above the mouth, but not actually touching it, a broader yellow line, finely edged with black, runs along the upper lip from the nostril to the corner of the mouth; there is a similar vertical line on the rostral and symphysial shield, which, being interpreted, means that a yellow black-edged line runs from the tip of the snout to the chin.

Very little is known of this handsome snake owing to the detestable climate of its home in Central America. There is no big game there to attract sportsmen, to whom our collections are so much indebted; and he is a bold naturalist who, in search of rare plants, insects, or reptiles, ventures into that fever-striken wilderness.

Third, *A. contortrix*.†—Easily to be recognized on sight by its coloration. The ground colour is a bright burnished light copper, with darker cross bands of a rich reddish brown, which are broad at the base and contract as they approach the dorsal ridge, thus giving the light interspaces the appearance of being broad on the back and narrowing on the sides; the head is generally lighter than the ground colour.

This snake, the copperhead, is probably the most dreaded creature in North America, as well as being one of the handsomest serpents known—that is, in my opinion; the general effect being more pleasing than the varied hues of other more brilliant snakes. It has been classified as *A. mokasen*, and as a good deal of error is connected with the name "mocassin,"‡ I will take this opportunity to endeavour to clear it up. There is the true mocassin, *Tropidonotus fasciatus*, a harmless snake of sombre colour; the water mocassin already described; and the upland mocassin or copperhead, which is smaller and more lightly built than its congener: the two last are often called the "mocassin" simply.

* Latin: "two-lined." † Latin: "twisting."

‡ Mocassin is pronounced "Mokkesin."

The only accident from a copperhead bite which I ever heard from an eye-witness, terminated fatally in a few hours, putrefaction setting in almost immediately after death.

Fourth, *A. acutus*.*—The sharp point which projects horizontally from the tip of the snout makes it impossible for anyone to mistake this pit-viper. The upper part of the head is very dark brown—the lower, yellow—the two colours being sharply divided by a black line which runs through the eye; the general hue of the body is a dark or light brown, with very dark diagonal cross bars which intersect each other on the dorsal ridge.

Very little is known about this serpent. There are, I believe, only a few specimens in this country, and for these we are indebted to the indefatigable Mr. Pratt, who obtained them in China. Those that I have seen show it to be a heavy, bulky snake; and as the biggest of these specimens is about five feet long, and about as thick as my arm, I imagine it to be considerably the largest of the group. I have been able to ascertain nothing at all about the virulence of the poison; but, I should think, from the length of the fangs and the size of the poison channel, that a bite would be very dangerous.

Finally, there are three species in which the upper labials are separated from the loreal pit, and the subcandals are in pairs.

First, *A. haylys*† (with which I combine for the purposes of this article the *A. blomhoffi* and the *A. intermedius*).—The snout is blunt and turned up; running from the eye along the temple is a dark band with lighter edges, which is a little broader than the eye; on the snout is a dark spot, on the top of the head are two more, and on the back of the head are two slanting streaks. Really about the best way of recognizing this variety that I can suggest, is a negative one. If it has not the characteristic marks of one of the other species, then it is a *halys*. It is a small, pale, dirty-looking viper, usually grey or brown, with no very distinctive features. It is found from the coast of China to the Caspian Sea, and is the only European pit-viper.

Second, *A. Himalayanus*.‡—Somewhat resembles the *halys*, but is much darker in colour. On close examination it can easily be distinguished by the size of the last two upper labials, which are very large and are merged into the lower temporals. I have always found a very thin black line, with a fine white edge, running from the eye to the corner of the mouth, surmounted by a band of a darker shade than the ground colour.

I have not been able to get much information about this snake, but it is probably not very dangerous. It is found at even greater heights than its near relation, the *Lachesis monticola*,§ specimens having been seen at an elevation of ten thousand feet.

Third, *A. rhodostoma*.‖—A light band runs from the eye to the corner of the mouth, below which is a broader dark streak with a black edging. This black edging skirts the upper border of the posterior upper labials in small curves or festoons; the colour of the lips, from which the name is derived, is pink or yellowish. The head, viewed from the side, somewhat resembles that of the *bilineatus*; but a closer inspection will show that the resemblance is only apparent, and an examination of the snout will clear up all doubts, as the *rhodostoma* has

* Latin: "sharp."

† A title formerly of a group of East Indian pit-vipers.

‡ Himalayan, latinized.

§ Latin: "living in the mountains."

‖ Greek: "rosy-mouthed."

no vertical line on the rostral shield. The ground colour is soft red, brown, or grey, with dark, angular, black-edged spots, very elegantly arranged.

In brilliancy and harmony of colour it is, perhaps, the most beautiful of the *Ancistrodon*, though I prefer the more sober copperhead; and it is probably the most venomous. An acquaintance of mine brought a very bad account of the *rhodostoma* from Java, to which island it appears to be confined; and Dr. Gunther relates that Kuhl saw a man succumb to the bite in a very few minutes, but I can find no record of any experiments with the venom. In this respect the *bilineatus*, which also lives under the line, may be a possible rival; but, as I have said, I can get no information—that is, reliable information—on the point. The peculiar virulence of the venom of the *rhodostoma*, which is not by any means a large snake, is rather remarkable, as the majority of the East Indian pit-vipers do not appear to be very dangerous.

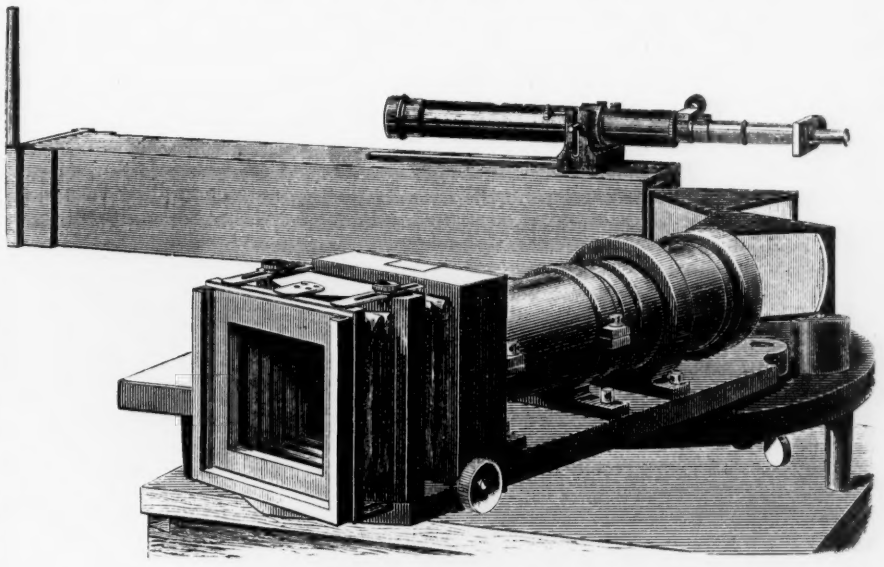
I have only suggested the lines for a rough-and-ready "recognizer," which might be applied to any family of serpents. At the same time I can assure those who are interested in ophidians that a more minute study of this or any other genus, will well reward the student; and that during its pursuit they will naturally and easily become acquainted with those more striking features which I have endeavoured to illustrate.

THE PRISMATIC CAMERA DURING TOTAL ECLIPSES.

By WM. SHACKLETON, F.R.A.S.

NOW that the last eclipse of the century is close upon us, and at nearly every observing station a prismatic camera is to be employed, it may be interesting to give a brief account of some of the results which the revival of its use during total eclipses has elicited for us. Just in the same way that Fraunhofer's method of observing stellar spectra has been applied to photographing the spectra of stars with amazing results, so the same method of placing a prism in front of the telescope and observing the sun when totally eclipsed has, with the aid of photography, given equally important information.

The prismatic camera as used during eclipses is simply an ordinary camera (with a lens of from two inches aperture and upwards) in front of which is placed one or more prisms, so that, instead of photographing the sun directly, the light has first to pass through the prism, which differentiates the composite light of corona, prominences, and chromosphere into the many monochromatic images of which it is composed; and if sufficient dispersion be used these are so separated as not to interfere with each other, but are perfectly distinct.



Prismatic Camera, used in Brazil.

The advantages of using this slitless spectroscope over one with a slit during a total eclipse is self-evident, for by it all the phenomena round the dark moon can be analyzed at once with a maximum aperture, whilst in the case of an ordinary spectroscope only the small portion which the slit crosses can be brought under observation.

That this is a desideratum one may see when it is remembered that it is only possible by intermittent glances to observe the eclipsed sun for about two hours in a lifetime. Fortunately, however, the chromosphere and prominences, which were enigmas for nearly two centuries, have, since the discovery of Lockyer and Janssen in 1868, been possible to observe and photograph without an eclipse.

When we come to the corona the story is a sadder one, for it must have been observed from the time of primeval man; indeed, we have hieroglyphical records of it by the ancient Egyptians and Babylonians, and yet we know least of all about this the greater bulk of the sun. Hence the prismatic camera, for giving us a large survey of its chemical constitution, is again the most advantageous instrument to employ.

Not only for quantity, however, but for quality also, is it paramount. When photographs are taken with a slit spectroscope it is really the slit that is being photographed, and any light, no matter how it reaches there, is what is being investigated. Generally an image of the particular part that is required to be studied is focussed on the slit by a condensing lens, and this gives the principal effect; but besides this there is a general illumination from all the other parts, for the light from these is scattered and reflected by minute dust particles in our atmosphere, so that in addition we have the integrated light from these superposed on what we wish to investigate, and, of course, the brightest of these extraneous sources gives the greatest additional effect.

In the case of the prismatic camera, however, it is only the real images of the eclipsed sun that are focussed on the photographic plate, and the general illumination of the atmosphere, although equally passing into the camera, has no definite outline, and therefore no image can be formed; so it is more scattered still by the prism, and only goes to

give a slight general fogging of the plate. Evidently, therefore, if we wish to truly sift out the light of the corona from that of the prominences the latter instrument must be employed.

Although the prismatic camera has been used during eclipses at various times since 1875, it was not until 1893 that sufficient dispersion and accurate focus were secured in order to make use of the differentiation referred to above, or it might be that the plates were not sensitive enough to record the exceedingly delicate monochromatic rings from the lower parts of the corona, which is the only part left sufficiently bright after the great deduction that must be made for the light giving only a continuous spectrum. During the total eclipse of 1893 photographs were taken in West Africa by Mr. A. Fowler, and in Brazil by myself, which showed that the coronal light gave rise to no H or K radiations of calcium—that the prominences on the sun at that time had no 1474 K light; and although this line, sometimes seen in eruptive prominences, might be accounted for by supposing that it really is the base of the corona which is being observed, or that coronal matter has got entangled with the great disturbances taking place, still in one such prominence during that eclipse no trace of it could be found. Again, in the eclipse of 1896, more than three years later, the photographs show the same thing; so we await with interest the results of the coming eclipse, to see if in passing from a maximum to a minimum sunspot period any change takes place in the constitution of the corona.

If a comparison be made of the K (calcium) and 1474 K rings with a picture of the eclipsed sun, it is clearly seen that 1474 K is truly coronal, and that H and K, which are identical with each other, are solely due to the prominences.

Had these facts been sufficiently well established in 1893, M. Deslandres might not have tried in vain to determine the rotation of the corona by photographing the relative displacement of the H and K lines on opposite limbs of the sun. In the last number of KNOWLEDGE it was stated that Mr. Newall is going to try to make the same observation, using a "bright line near G, of whose coronal nature there can be no doubt." Let us hope he has consulted the records of the prismatic camera before doing this, for although one such line was tabulated at λ 4232·8 by Schuster in 1886 as being the brightest in the photographical region, the results of 1893 and 1896 show that a bright line near H, λ 3987, is more intense than this, and in fact is the next strongest line to the coronal line (1474 K) itself. This will be seen on examining the photograph taken near mid-totality, and reproduced here in the plate by the kind permission of the Royal Society.

So far we have examined the capabilities of the prismatic camera for giving us information about the parts of the sun comparatively well removed from the photosphere; let us now turn our attention to see what can be done with it for the investigation of those vapours which lie closer in, in order to test Kirchoff's theory "that the absorption which produces the dark Fraunhofer lines takes place in a thin stratum, or *reversing layer*, as it has often been called, adjacent to the photosphere."

In a total eclipse of the sun, at the moment the advancing moon just covers the sun's disc, the solar atmosphere of course projects above the dark edge, and at that moment the reversing layer will be isolated for only a very few seconds. If, now, at this precise instant, a photograph be taken with the prismatic camera, we shall have the spectrum of this shallow layer, chromosphere and corona; but from the form of the arcs and their appearance or non-appearance in later photographs, we shall be able to separate the integrated effect into its individual parts. From the very nature of this layer and the inequalities in the moon's position, the difficulties in the way of making the exceedingly fine adjustment of placing a *slit* upon this *point* of disappearance are almost insurmountable; in fact, so great are they that it was not until the application of the prismatic camera, which requires no such nicety of adjustment, that there was any permanent record of this low-lying stratum.

Except at an eclipse it has not yet been found possible to observe this bright line spectrum, because it is overpowered by the aërial illumination of our own atmosphere so spectroscopists are the more anxious to make the most of every eclipse to settle at least this one point. With this end in view many prismatic cameras have been directed to the eclipsed sun, but it was not until 1893 that anything like the base of the sun's atmosphere was photographed.

The difficulties of placing a slit on a point have been mentioned previously, but not only is there that to contend with, but also, no matter what instrument be used, the exposure must be made at the precise moment the sun's disc is covered. To do this, Sir Norman Lockyer, during the eclipse of 1896 in Norway, instituted a "running plate," which took a series of snapshots just before and going on till the critical moment had passed—in fact, a sort of kinematograph arrangement. Unfortunately, however, the weather was unfavourable to let us see what results this method would give. Mr. Evershed, also, in clouded-out Norway, and myself, in Novaya Zemlya, relied more on the exact determination of the proper instant, and then making a short exposure. What such a photograph is like, and how far it agrees with a reversed solar spectrum, can be gathered from the plate, which is a reproduction of the Novaya Zemlya photograph. Of course any comparison must be made with a spectrum obtained by a similar instrument, for it would obviously be fallacious to compare a spectrum taken with only a moderate-sized spectroscope, making clear to us only a few hundreds of lines, with such a spectrum as that taken with a Rowland grating, which reveals *in the same sunlight* as many tens of thousands of lines. Therefore, the only way of absolutely proving that every fine dark line is reversed would be to photograph this layer with a Rowland grating, which, with our present appliances and the short duration of visibility, is nearly impossible; but this is to be tried by Prof. Michie Smith during the forthcoming eclipse. Still, notwithstanding these difficulties, the investigation of the lines in the photograph is proceeding at the Solar Physics Observatory, South Kensington; but, probably, before it is finished we shall have many such photographs, with more powerful instruments, from India, where the sun will be bombarded, not by one only, but by at least half a dozen prismatic cameras.

With such possibilities in an eclipse, no wonder during such times that the sun monopolises the attention of astronomers, not only for the secrets he has to divulge of himself, but also for the key he may possibly give to cipher the constitution of other countless suns more remote.

NOTES ON COMETS AND METEORS.

By W. F. DENNING, F.R.A.S.

COMETS.—1897 has afforded only one new comet—that discovered by Perrine on October 16th. When first seen, the comet was placed in the south-east region of Camelopardus and moving north-west; it has since traversed Cassiopeia, Cepheus, and Draco. Early in January,

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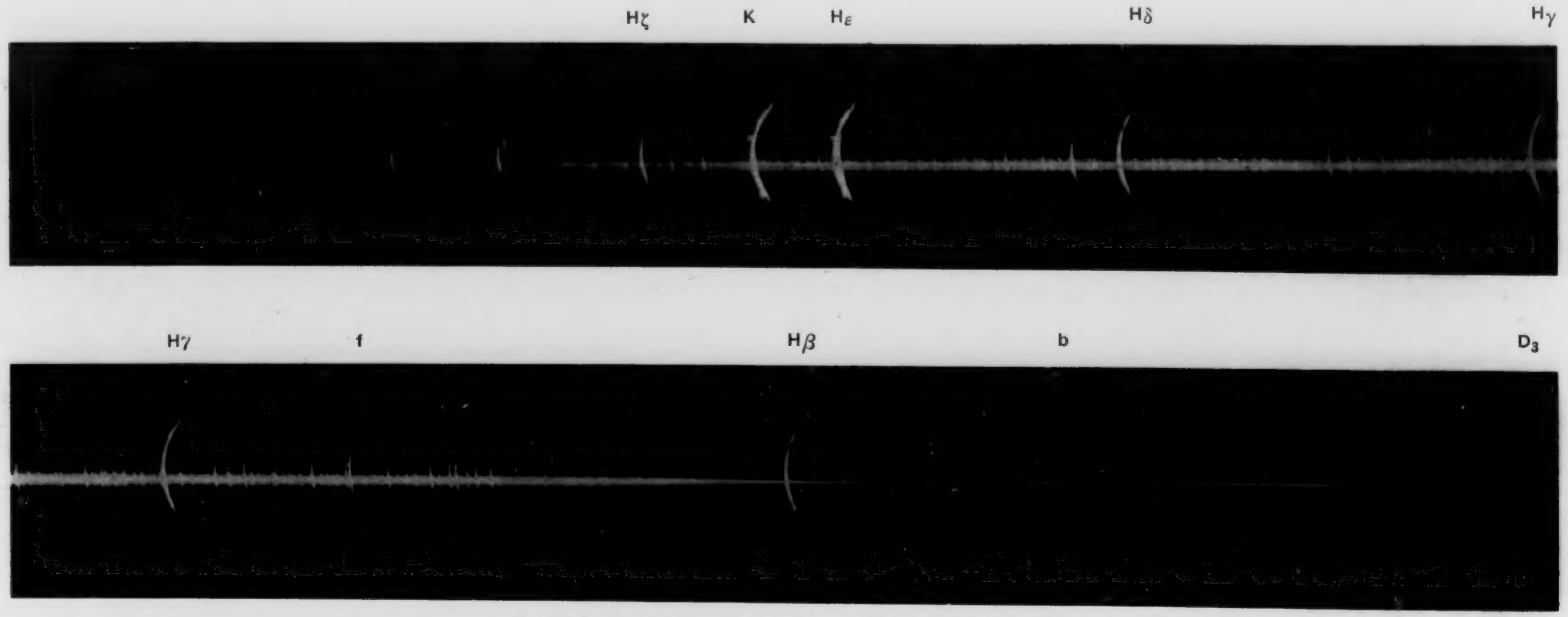
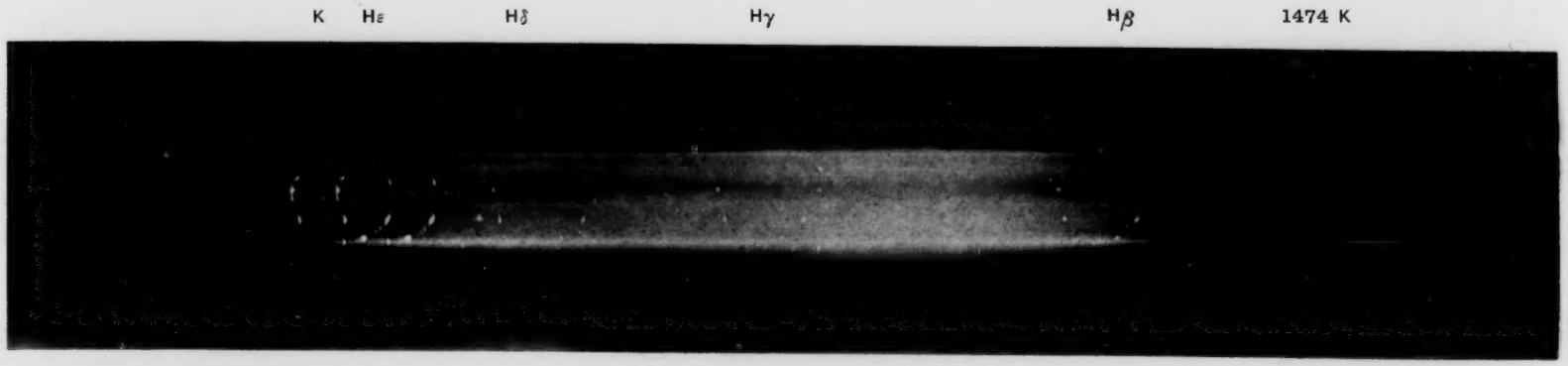


Photograph of the "Reversing Layer," in Two Portions.



Photograph near Mid-Totality, showing Coronal Ring in 1474 Light.

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1898, the comet will be almost stationary at a point six degrees south by east of γ Draconis, its apparent displacement being only ten minutes of arc per day. Its brightness will be 0·4, as compared with that (adopted as 1·0) at discovery. The elements show its inclination to be sixty-nine degrees, whence we may infer that its orbit does not deviate much from a parabola. The physical aspect of the comet has been interesting, for it presented a nucleus, coma, and tail. On October 25th, as observed by Mr. F. W. Longbottom, at Chester, with an eighteen and a half inch Calver, the total length of the tail was twenty minutes of arc, and the comet was estimated not quite equivalent in brightness to a ninth magnitude star. The tail was tapering, not fan-like, and stars showed brightly through it on October 30th.

Several periodical comets were due in 1897, but only one of these was observed, viz., D'Arrest's, which was picked up by Perrine on June 28th, more than a month after its perihelion passage. Spitaler's comet of 1890, and Tempel-Swift's comet of 1869-80, also returned to perihelion in the spring, but the conditions were too unfavourable for them to be observed.

In 1898 five periodical comets are due. Pons-Winnecke's arrives at perihelion in March, Encke's in May, Swift's (1889, VI.) and Wolf's in June, and Tempel's (1867, II.) in September. The circumstances attending the return of these several objects are by no means good, and in most cases they are likely to escape observation unless some of the large telescopes at present in use are employed in searching for them.

Mr. C. Hildebrand gives the following ephemeris of Pons-Winnecke's comet :—

1898.	R.A. H. M. S.	Dec.
January 2 ...	15 18 24 ...	− 3° 55·3′
,, 6 ...	15 32 53 ...	− 4° 52·3′
,, 10 ...	15 47 58 ...	− 5° 49·3′
,, 14 ...	16 3 40 ...	− 6° 46·1′
,, 18 ...	16 20 0 ...	− 7° 42·1′
,, 22 ...	16 36 59 ...	− 8° 37·0′
,, 26 ...	16 54 36 ...	− 9° 30·3′
,, 30 ...	17 12 49 ...	− 10° 21·3′

The diurnal motion is therefore about one degree eastwards, and during the month it carries the comet through Libra and Opiuchus.

It is remarkable how the stream of cometary discovery runs continuously on. No sooner do one or two successful sweepers leave the field than others step in and pursue the work. Messier, Mechain, and Caroline Herschel, in the latter part of the last century, were succeeded by Pons early in this. He in turn was followed by Tempel, Winnecke, Borrelly, Coggia, and Swift. Then Barnard and Brooks almost monopolized the field for twelve years. To-day Perrine may be regarded as the comet finder *par excellence*, for he has worthily emulated Barnard's former discoveries at the Lick Observatory, and has found five new comets within the last three years.

METEORS.—The November Leonids of 1897 very generally disappointed expectation. Cloudy weather and moonlight were certainly responsible in a great measure for this, and, moreover, there is no doubt that an exaggerated idea as to the probable intensity of the display was encouraged by the majority of those who looked for it. On the basis of reports supplied by eye-witnesses of the phenomena of 1831 and 1864, it was predicted that the shower might equal a rich return of the August Perseids, furnishing, perhaps, one hundred meteors per hour for an observer. The time of maximum was mentioned as uncertain, but as sure to be included in the mornings of November 14th and 15th. As events happened the first of these periods was partly clear, while the next morning was cloudy nearly everywhere. Meteors were comparatively rare during the whole night of the 13th, and clouds hid those visible during the following night. In France and America, as well as in England, the experience appears to have been very similar. At a few places, where the sky was clear on the morning of November 15th, the Leonids were both numerous and brilliant. At Dumfries, two observers (ignorant of the expected display) were struck at the extraordinary prevalence of shooting stars, and estimated the visible number as ten per minute. Another observer, at Loughborough, saw a considerable number of meteors, including five of great brilliancy, and the time of their maximum frequency seems to have been at about 5 A.M. A third observer at Dumfries had his attention arrested by the surprising frequency of meteors, and states that more than five per minute were perceptible. At Derby meteors were so abundant as to cause special remark. Eighteen fine ones were noticed between 3h. 30m. and 5h. A.M., and these included two nearly as bright as the full moon. Prof. Lewis Swift also reports from Echo Mountain, California, that "the Leonids made their appearance on the morning of November 15th, ninety-seven having been counted by one person." From these and other corroborative accounts it is certain the shower was quite as abundant as expected, and, at places were the sky was clear, sufficiently striking to attract particular attention notwithstanding the moonlight. The idea that the display failed to present itself is due to a misapprehension. On the preceding night the experience of observers seems to have been practically unanimous in describing the meteors as scarcely more numerous than on an ordinary November night.

Prof. E. C. Pickering, of Harvard, states that the observers at the observatory at Cambridge, Mass., counted only ninety meteors during the night of the 13th, but that these were nearly all Leonids. Prof. O. Stone, of Richmond, Va., says that on November 13th several meteors were seen from the direction of Leo; one of them was several times brighter than Venus, and travelled along an arc of ninety degrees, leaving a streak forty degrees in length. The following night was cloudy, and nothing could be seen. Prof. Barnard, at the Yerkes Observatory, saw nothing on November 13th and 14th, as clouds and rain prevailed each night. In England a few meteors were seen on November 13th, but they call for no special remark. Dr. W. J. S. Lockyer, of South Kensington, recorded a number of paths, and others were registered by Mr. Salmon, of South Croydon, and Mr. Besley at Westminster. These materials show that, though the Leonids returned on the night of November 13th, the shower was very feebly represented. These and many other observers were baulked by clouds in their efforts to secure observations on the night of November 14th. At Bristol the sky was overcast throughout, though at 5 A.M. on the 15th the clouds became thinner, and the moon shone faintly through them, but no meteors could have been observed unless they were of great brilliancy.

Observers of meteors will be interested in watching for the January shower from Quadrans, usually visible on the 2nd of that month. The moon will, however, partly interfere in the evening, and the best time to observe the display will be between 3 and 6 A.M. on January 2nd. The radiant is at 230°+52°, and the shower is often a conspicuous one, furnishing rather swift, long-pathed meteors.

XUM

RICHARD PROCTOR'S THEORY OF THE UNIVERSE.

By C. EASTON.

RICHARD PROCTOR, the founder of this magazine, amongst the other services that he has rendered to science, deserves the credit of being the first to offer a solution of the problem of the structure of the heavens by studying it from a general point of view, whilst at the same time basing his theory on direct observation. Huyghens, Thomas Wright, Kant, Lambert, and others, had already touched on this great problem, but they had to content themselves with reasonings; they misused the arguments *per analogium*, having very few facts to go upon. The two Herschels collected an enormous quantity of facts and precise data relating to the problem, but they were reluctant to draw from them any definite conclusions. Sir William Herschel himself abandoned a considerable number of his early ideas on the structure of the heavens, although he did not declare in a definite manner what changes must be made in it. As for his son, he demonstrated the untenability of the *cloven disc theory*, and of the fundamental suppositions made by his illustrious father, especially in the face of the evidence drawn by Sir John himself from his telescopic observations of the Milky Way. Contrary to what has been often said, Sir John Herschel has stated expressly and exclusively—at least in his books—the theory generally* attributed to him of the galactic ring, although he seems to have found in this theory the fewest obstacles to the explanation of the phenomenon.

From the beginning, Proctor insisted, when discussing the conceptions of Sir John Herschel, that neither the cloven disc theory nor the theory of a galactic ring could adequately explain the observed facts. In the case of the second theory, Proctor only indicates its insufficiency in a general manner. Even the principal features of the Galaxy, he says, offer too great difficulties for the annular theory, and he boldly sketches a more complicated figure, which, he says, replaces with advantage Sir John Herschel's theory explaining the principal details of the Milky Way.

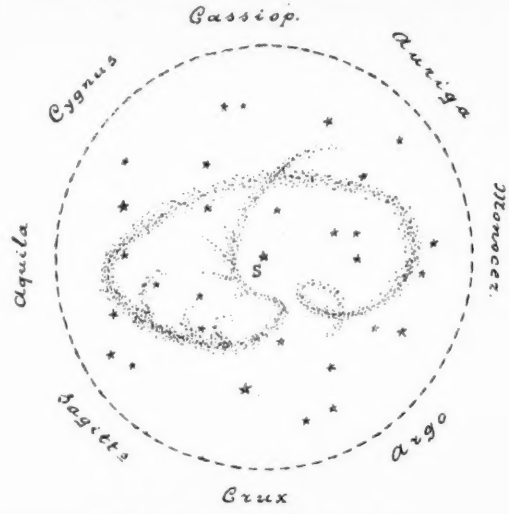
Whilst recognizing that the extreme complexity of the details in the Milky Way may never perhaps allow of a complete solution, Proctor was convinced that "the bolder and more striking features of that circle may be studied with a better hope of their being successfully interpreted."

He has been reproached with too much audacity, and, indeed, one hesitates to subscribe with Proctor to "the spiral curve, which [as] depicted seems so satisfactorily to account for several of the more striking features of the Milky Way as to suggest the idea that it corresponds somewhat closely to the real figure of that star-stream." But it seems to me that the advantages of his researches are much superior to the disadvantages. Those who approach with hesitation and prudence by far other ways will not be led astray by the errors of Proctor's method; and, on the other hand, pioneers of science such as he exercise a great moral influence—their digressions, though sometimes overbold, refresh and stimulate the zeal of others.

This gigantic arch of the Milky Way, spreading out before all eyes the sublime enigma of its starry ramifications, seems to defy the indefatigable seekers bending over their calculations. Let others strive to draw some evidence of the aspect of the Milky Way from its chief outlines.

However, Proctor would doubtless himself recognize to-day that his theory does not now correspond with the actual state of science; and it is strange that in treatises of astronomy his well-known drawing is reproduced as if the theory could still be accepted, although more than one judicious remark of Proctor's preserves his reputation. At the period when he formulated his theory, Proctor had not at his disposal, in short, any but the results obtained by the two Herschels and by F. G. W. Struve; and, besides, the work of the latter was soon reduced by Encke to its just proportions—that is to say, to a negative result, or one nearly so. Almost all the modern work in this branch of astronomy has been done since Proctor's time—that of Heis, Houzeau, Gould, Celoria, Kapteyn, Ristenpart, Plassmann, etc.; and in particular, though basing his researches on the constitution of the Milky Way, he could not consult either the admirable photographs of Barnard, Wolf, Roberts, Russell, nor the modern drawings of it—that is to say, that he possessed scarcely any facts about the whole northern half of the zone.*

Also, the explanation furnished for the figure imagined by Proctor could not be considered as satisfactory to-day,



The Milky Way according to Proctor.

even for the main lines of the Galaxy. Many of his observations, however, are still valuable. When he says that where the line of sight is directed tangentially to either loop, the Milky Way may be expected to have greater width than elsewhere, he furnishes the best explanation of the curious fan-shaped expansions of the Milky Way on each side of the no less remarkable gap in Argo. His explanation of the Coal Sack in Crux—"the apparent inter-

* "... the Galaxy, which ... would come to be regarded as a flat ring, *or some other re-entering form* of immense and irregular breadth and thickness ..." (Sir John Herschel, "Outlines," § 788.) He prefers to represent the Milky Way as of an annular form, but he takes care not to pronounce definitely on this. "... an impression amounting almost to conviction that the Milky Way is not a mere stratum, but annular; *or at least* that our system is placed within one of the poorer or almost vacant parts of its general mass ..." (Mary Somerville, "The Connexion of the Physical Sciences," 1846, p. 419.)

In speaking of the lateral offsets which quit the main stream of the Milky Way, and which he regards as the "convexities of curved surfaces viewed tangentially, or planes seen edgeways" ("Outlines," § 792), he evidently does not trouble to bring them into accord with the theory of a galactic ring.

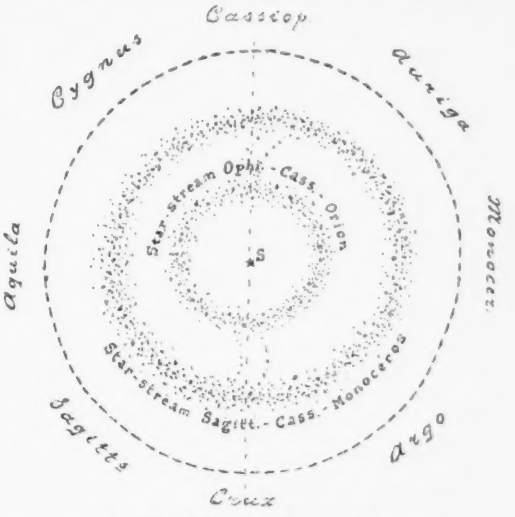
* Sir John Herschel, although he described with much particularity the southern half of the Milky Way, treated rather lightly the northern parts. Thus he says in his "Cape Observations," p. 386, speaking of the region in the Eagle, which is nevertheless curious: "After which [λ Aquilæ] this main stream runs northward through Aquila without any further distinguishing feature. ..."

crossing of the two contorted streams which really are at different distances from the eye"—is possible but not very probable, I think, after the evidence furnished lately by Gould and Russell. I do not think it was necessary in this case to turn aside from Sir John Herschel's opinion that the Milky Way in the neighbourhood of the Coal Sack is just "a distant mass of comparatively moderate thickness, simply perforated from side to side," or as an oval vacuity which is seen "foreshortened in a distant foreshortened area." ("Outlines," § 792.)

There is doubtless much truth in Proctor's supposition of the branches which are detached from the main stream of the Milky Way, and which penetrate into the neighbourhood of the sun, and of the less important ramifications which spring up at different points of the galactic course. But as regards the general reasoning followed by Proctor in comparing his figure with the aspect of the Milky Way, he is fundamentally in error; and this is an interesting point, since Sir John Herschel had already made a similar mistake. Herschel says that the brilliant and well-defined part of the Milky Way about Argo and Crux "conveys strongly the impression of a greater proximity"; and he deduces from this that the sun occupies an excentric position in the interior of the Milky Way, which is nearer to the southern than to the northern part of its circuit. Taking up this argument and amplifying it, Proctor admits that the stream grows gradually fainter with increase of distance towards Canis Minor and Monoceros; and in speaking of the brilliant portions of the Galaxy in Aquila and Sagittarius he is satisfied that "this part, which is so very bright, corresponds to the part which my spiral brings *so very near* to the sun."

But we should see precisely the opposite in this case. Sir John Herschel and Proctor have been too much taken up with the idea of a stream, of a "distant mass," which they represent as continuous, like a band of cloth, whose details are perceived with more clearness the nearer they are. But the phenomenon of the Galaxy is of quite a different nature. As long as the brightness of each individual star is of great importance, and their mutual distances which we see projected are insignificant, the reasoning of Sir John Herschel and of Proctor holds good. But it is, above all, the closeness, in projection, of the small stars in the Milky Way which produces the optical phenomenon of a galactic gleam. The individual brilliancy matters little. This can be easily demonstrated. From the gauges of Sir William Herschel and from the star-counts of G. Celoria, it follows that the number of stars sufficiently brilliant (seventh to eleventh magnitude) which take part in the formation of the lacteal light, is much more considerable in the region of Monoceros than of Aquila, and in spite of the fact that the Milky Way is, without gainsay, much more luminous in the latter portion of the sky than in the former. Then, the abundance of stars in certain lacteal regions (Scutum, Cygnus, etc.) is so great that the relatively bright stars form but an insignificant part of it distributed here and there among the multitudes of small stars. But, whatever may be the number of stars necessary for this, the stars are sufficiently near each other in perspective for their collective light to produce a strong enough impression on the extremity of our optic nerve and give us the impression of the lacteal gleam—an impression that could not be produced by stars each much more brilliant than the others united, but their projections too distant for their images to fall on the same nerve bundle in the retina.

The same thing is seen when a celestial object is resolved into stars which had until then appeared nebulous. Thus the parts of the Milky Way which are nearest to us would appear by the same rule vague, large, and rich in stars relatively bright, whilst the distant portions of the zone would appear more crowded and better defined—more luminous in themselves, though numbering fewer brilliant stars. One could easily represent this appearance by



The Milky Way according to Celoria.

imagining oneself within a huge circle of trees, nearer to one part of the circumference than to the rest. In the near part the trees do not form a continuous band, whilst they are confounded in one straight dark line in the further portions of the circle.

I have written at some length on this point because it undermines the reasoning of Proctor in more than one particular, and also demonstrates that one remark of Sir John Herschel, often quoted, rests on an erroneous argument, and that to my knowledge these points have been raised before.

After what I have just said it would be superfluous to criticise in detail the "spiral" of Proctor. For the rest, even if they furnished a perfect explanation of all the principal features which Proctor finds in the Milky Way, they could no longer serve, now that the principal features of the galactic zone in the two hemispheres—thanks to the drawings and to the photographs,* and in despite even of the differences that may be perceived there—appear to us under quite a different form. Must we, then, return to the theory of a cloven disc or to that of a galactic ring? Certainly not. Proctor has, without doubt, been right in giving up these premisses: that the theory of a stellar stratum in form could not be defended in these days, and that the phenomenon of the Galaxy is due to a distribution of the stars of a much more complicated character than could be produced by a ring, however irregular.

Without entering into details which would take too much space here, I hope to give a summary of what has led up to the result that the most modern researches (after Proctor) have established with sufficient certainty.

The visible universe, stars and nebulæ (with the exception of nebulæ properly so called), is extended in a flat layer irregularly condensed. The stars differ extremely, not only as regards their volume, but also as regards the

* Drawings of Heis, Houzeau, Davis and Thome (Gould), Boeddicker, Easton, and others; photographs of Barnard, Wolf, and Russell. The readers of KNOWLEDGE have often had prints of these admirable photographs of the Milky Way.

intrinsic brightness of their surface. The mode of distribution of the stars is not the same in different regions of the stellar layer, but the distribution of the great stars is not independent of that of the small ones. The stars of the spectral type—named "solar type"—are condensed about a point which, in comparison with the extent of the whole system, is not very far removed from the sun.*

Proctor looked upon the Milky Way as "the condensed part of a spiral of *small* stars" amidst the sidereal system. This theory is incompatible with the results recently obtained, in particular with those of Kapteyn and of my own concerning the distribution of stars of differing magnitudes in some parts of the Milky Way (*see* KNOWLEDGE, August, 1895). In the galactic belt the large and small stars are most certainly intermingled.

But modern researches have not yet touched upon a new theory of the Milky Way—a theory which can at least explain, as Proctor wished to do, the bolder and more striking features of the Milky Way. Giovanni Celoria alone, at the Observatory of Brera in Milan, has ventured as far as could be at his time (1878). From his painstaking and most interesting researches† he did not evolve a complete theory, but the comparison of his star-counts with the gauges of William Herschel and the "Bonn Durchmusterung" led Celoria to conclude that the "Milky Way is composed of two branches, two distinct rings, of uninterrupted circumference. One of these rings is represented by the continuous feature of the Milky Way, crossing the sky in Monoceros, Auriga, Sagitta, and Aquila; the other begins in the brilliant stars of Orion, passes through the Hyades, the Pleiades, Perseus, Cygnus, and ends in Ophiuchus. The two rings cross each other, and are perhaps confounded in one system in the constellation of Cassiopeia; and separating, one part passing through Cygnus and the other through Perseus, they make an angle of about nineteen degrees."

I do not need to say that the second ring of Celoria, crossing Orion and Ophiuchus, is identical with the belt of bright stars of Sir John Herschel and of Gould; but, in the course of his research, Celoria found that there existed in this region a veritable galactic branch, with many stars relatively brilliant although telescopic, and few stars of the inferior order of brightness—at least in the sections studied by the Italian astronomer.

Although there is doubtless much truth in the conclusions drawn from this great work, it is impossible, in the actual state of our knowledge of the composition of the Milky Way, to accept the "due anelli distinti, nè mai interrotti nel loro corso" of Celoria. Even if the two rings are tenable, it must be recognized that there are lacunæ, interruptions, and, in a word, manifest complications.

If one would rest on the solid ground of fact, one cannot go beyond this conclusion—at least as regards the great problem of the structure of the heavens: great irregularities of detail, traces of at least partial regularity in the principal features. But I hope in another paper to venture a little further in this tempting region, without, however, quitting a firm hold of observed facts.

* For further particulars, *see* among others—Gould, *Uranometria Argentina*, 1879; Schiaparelli, *Pubbl. del R. Osservatorio di Brera*, XXXIV.; Celoria, *idem*, XIII.; Plassmann, *Jahresbericht d. Westfahlia, Pr. Vereins*, 1886; Ristenpart, *Ber. Sternw. in Karlsruhe*, 1892; Kapteyn, *Verst. Akademie v. Wet. Amsterdam*, 1892 and 1893; Gore, "Visible Universe," etc.; Ranyard, KNOWLEDGE, June, November, 1894; Maunder, KNOWLEDGE, February, November, 1895. February, 1896; Easton, *Astron. Nachr.*, 3270. *Compare also* KNOWLEDGE, October, December, 1891; May, 1892; April, 1893; October, 1894; January, August, 1895.

† Giovanni Celoria. "Sopra alcuni scandagci del cielo et sulla distribuzione generale delle stelle nello spazio." *Pubbl. del R. Osserv. di Brera*, XIII., Milano, 1878.

Conducted by HARRY F. WITHERBY, F.Z.S., M.B.O.U.

A NEW BRITISH GULL.—The Mediterranean Herring Gull (*Larus cachinnans*).—The past autumn has been exceedingly unproductive of the rare migrants which usually visit the east coast; it is therefore especially gratifying to be able to rescue from oblivion a rare bird which has been unrecorded since the month of November, 1886. Mr. Cole, the well-known bird preserver, of Norwich, recently called my attention to a Gull which was shot on Breydon Water, near Yarmouth, on the above date, by the noted gunner John Thomas, and sent to him. The late Mr. Henry Stevenson examined it in the flesh and stated his opinion that it was an example of the Mediterranean Yellow-legged Herring Gull (*Larus cachinnans*); but, somehow, it passed out of notice till Mr. Cole called my attention to it recently, when a careful examination of the bird convinced me that Mr. Stevenson's opinion was correct. This has since been confirmed by Mr. Howard Saunders. The bird is a male by dissection, and differs from the common Herring Gull in having the mantle darker, the base ring round the eye deep orange-red, and the legs lemon-yellow. The resemblance to the common Herring Gull is, however, so great that it might easily be overlooked. The month of November seems to be a very unlikely one for the occurrence of this southern species on our coast, but I find that the weather at that time was exceptionally mild and pleasant. It is also remarkable that in the following month another Mediterranean species, *Larus melanocephalus*, was killed in the same locality.—THOMAS SOUTHWELL, Norwich.

FERRUGINOUS DUCK (*Fuligula nyroca*) IN WEST MEATH.—Mrs. Battersby, of Cromlyn, informs me that a bird of this species was shot by Colonel J. R. Malone, at Baronstown, Balinacarghy, West Meath, on January 17th, 1897. The bird was stuffed by Mr. E. Williams, Dame Street, Dublin, who informs me that it was a mature female. This specimen does not seem to have been recorded before, and, as the species has only been identified four or five times in Ireland, the occurrence is worthy of record, although the bird was shot a year ago.—H. F. W.

A NORFOLK GREAT BUSTARD.—Through the kindness of Prof. Newton I was enabled a few months ago to purchase a remarkably fine male example of the old local race of this magnificent bird. The result of my inquiries amply established its history, which is briefly as follows:—The bird was shot on Swaffham Heath about the year 1830 by a gentleman named Glasse, who then resided at Vere Lodge, Raynham, near Fakenham, Norfolk. It remained in his possession and in that of his daughter until, on the death of the latter at Bournemouth, early in the present year, it was sold by auction with the rest of her effects, and is now in the collection of Mr. Connop, of Rollesby Hall, Great Yarmouth. This superb old male,

in magnificent plumage, is even larger than the grand male in the beautiful group of seven of these birds in the Norwich Castle Museum; and from the date of its death is not unlikely to have been the last male of the Swaffham drove, the females of which were not finally exterminated until the year 1838, when the last of the Norfolk-bred Bustards was killed.—THOMAS SOUTHWELL, Norwich.

WAXWINGS (*Ampelis garrulus*) AT SCARBOROUGH.—There are quite a lot of Waxwings at the present time (November 4th, 1897) in this locality, upwards of half a dozen having been shot and sent to me for preservation. The birds which have been captured were found feeding on the berries of the mountain ash and alder, and were so tame as to allow their executioners to walk beneath the bush whilst they sat on the top of it quite undisturbed.—J. MORLEY, King Street, Scarborough.

VARIETY OF THE COMMON GUILLEMOT AT SCARBOROUGH.—A beautiful variety of the common Guillemot was caught on December 4th, 1897, in Scarborough Harbour. Its head and entire under parts are white, whilst its back and wings are of a whitey-brown colour, and its bill, legs, and feet yellowish white. A bird of this description is extremely rare; a similar one was obtained a few years ago at Filey. The writer has visited Speeton Cliffs for many years during the breeding season, and amongst the hundreds of thousands of birds that annually resort there for breeding purposes, has seen but one creamy coloured Guillemot.—J. MORLEY, King Street, Scarborough.

HOUSE SPARROWS AND PIGEONS.—That Sparrows should singly pursue Pigeons—white birds for preference—and snatch feathers from the breast and sides, is, I imagine, no news to the majority of your readers, though I have more than once met with doubt when alluding to the practice. Never before, however, have I observed this robbery in mid-air before March; and it may seem to you a sufficiently interesting sign of the abnormal state of things this year—though to-day is cold enough—that I have this morning seen four feathers taken in this way from the white Pigeons next door. This haste for warm lining for the nest points, without doubt, to very forward domestic arrangements.—F. G. AFLALO, Bournemouth, December 4th, 1897.

[Sparrows commonly take feathers to their roosting places during the winter. The fact of their carrying feathers about at this time of year does not, therefore, necessarily point to early nesting.—H. F. W.]

OCCURRENCE OF A COLONY OF JACKDAWS HAVING DOMED NESTS.—My boys having told me that for several years they had found in the neighbourhood of Moddershall, Staffordshire, Jackdaws with nests like Magpies, on the 14th May Dr. McAldowie (author of "The Birds of Staffordshire") and I went to verify this strange occurrence. On our arrival at the spot indicated to us—a group of Scotch firs on a bank rising from a large pool—we found five large nests, and saw flying round overhead four old Jackdaws. On a later day, accompanied by one of my sons, I paid another visit to the colony. One nest was placed at a height of fifty-eight feet, in the highest fork of a tree. The nest was a very bulky one, two and a half feet in diameter and of a like depth, constructed of sticks: the nest cavity, which was ten inches across, being filled with cow hair and wool, of which there was a large quantity, and the whole covered and protected by a strong dome of thorny sticks, which a hedge at the side of the plantation had no doubt supplied, it having been recently cut and the cuttings left on the ground. There was one entrance at the side of the dome. The nest was empty, and from the absence of dirt and castings was evidently one of this year. Two other nests were placed in similar positions in other trees; one contained four young birds about a fortnight old, and the other was an old one. We found some egg shells under another tree, but did not climb it. I have made inquiries, but cannot hear of these trees ever having been occupied by Rooks; and the absence of earth and clay, with which Magpies invariably line their nests, makes it improbable that these birds were the builders. I am therefore compelled to believe that the Jackdaws built these nests. There are two other colonies of Jackdaws in the neighbourhood, both in sandstone cliffs, and a mile or so from the colony I have described. I shall be extremely obliged if any of the readers of KNOWLEDGE who may have met with a similar occurrence will describe it.—W. WELLS BLADEN, Stone, Staffs.

[It seems difficult to prove that these nests were not old Magpies' nests relined and restored by the Jackdaws. The earth and clay of the Magpies' nests would probably wash away in the course of time. It would be very interesting if, during the coming spring, Mr. Bladen should be able to incontestably prove that these Jackdaws do build domed nests.—H. F. W.]

Notes on an Expedition to Rockall. By R. Lloyd Praeger, B.E. (*Irish Naturalist*, December, 1897, pp. 309 to 323.)—This is a brief diary of ten days spent in twice visiting the oceanic islet of Rockall, and forms the "day-by-day experiences of the party sent out in June, 1896, by the Royal Irish Academy, to investigate the natural history of this little known and inaccessible rock and of its vicinity." Unfortunately the expedition was unsuccessful in attaining its main object—that of landing upon the rock.

All contributions to the column, either in the way of notes or photographs, should be forwarded to HARRY F. WITHERBY, *at 1, Eliot Place, Blackheath, Kent.*

NOTE.—The first issue of KNOWLEDGE containing British Ornithological Notes was that for October, 1897.

Science Notes.

MR. WALTER SICHE, the traveller and florist, has returned from an expedition to the Cilician and Cappadocian Taurus with a large number of alpine plants, and ten thousand examples of various species of the asphodel family, with varieties of fritillary, galanthus, colchicum, iris, and many other plants. Mr. Siche has been the means of introducing many new flowers to the domain of English horticulture.

Lieutenant Peary, of the United States Navy, in his recent address before the Royal Geographical Society, said that to-day Greenland had no interior—it was simply a great white snow shield. On that frozen surface the traveller sees but three things—an infinite expanse of snow, an infinite expanse of sky, and the stars. One thing of interest to glacialists which he mentioned was the transportation of snow by the wind, which was almost always blowing there. Referring to his location of the famous iron mountains of Sir John Ross with their nuggets of iron, he intimated that the Eskimo legend in regard to these nuggets was that they were originally an Eskimo woman and her dog, which were thrown out of high heaven and landed in that inhospitable region. A woman six thousand pounds in weight was the source from which the Eskimo obtained their iron supply for generations!

Sir John Lubbock, lately lecturing on "Ants," said that the lives of these creatures were much longer than is generally supposed. He had kept many for several years, two queens having reached the age of fifteen years, and

these were by far the oldest insects on record. Several species kept aphides which they milked like cows; and he had found that in the autumn they collected the eggs of the aphides and kept them all through the winter, although they were of no use, and the young aphides hatched from them gave none of the sugary fluid till the following May or June, so that the ants showed more thrift and forethought than many human beings. Their instincts, though so wonderful, were very limited; and yet, when the ants were watched building their nest, feeding their young, tending their domestic animals, and, in some cases, their slaves, it was difficult to believe that they were unconscious automata.

We are pleased to observe that a scheme is shortly to be submitted to Parliament involving the expenditure of upwards of three millions for the better housing of the national collection of art treasures in and about South Kensington Museum. The Bill for this purpose is to be brought before Parliament next Session, and there will shortly commence to be built a series of exhibition rooms and galleries, to concentrate in one area the many works of art and objects of interest now scattered in various extempore structures. It is to be hoped that among the innovations there will be a replacement of those wooden huts—called by courtesy an observatory, but bearing a much closer resemblance to a hen farm—by something more in keeping with the long purse of a Government with such resources as ours.

A great undertaking, namely, the measurement of a degree of latitude in the Polar regions, leading to a more exact knowledge of the form of the earth,* appears to be on the eve of accomplishment. The solution of this question has long been the chief aim of Swedish Polar exploration, and Prof. E. Jaderin has now proposed to the Government for a preliminary expedition to be sent to Spitzbergen next summer, and that Russia should be invited to co-operate in the final measurement of a degree in 1899 and 1900. The task of the preliminary expedition—which it is intended should start in May and return in September—would be to complete the investigations already made as to the facilities for the necessary triangulation, to reach the summits of hitherto unclimbed mountains, to set up signal posts, and so on.

Dr. Campbell Morfit died last month at South Hampstead. An American by birth, he had for many years past been a London resident. He was the author of "Chemical and Pharmaceutical Manipulation," "Arts of Tanning and Currying," "Oleic Soaps," and, with Dr. James C. Booth, was joint editor of the American "Encyclopædia of Chemistry"; and in the industrial utilization of waste products, as well as the chemistry of food substances, his researches have been of the utmost service to the general public.

The November Number of the "Archives of the Roentgen Ray," which is now the organ of the Roentgen Society of London, contains an excellent report of the presidential address delivered by Prof. Silvanus Thompson, F.R.S., to the Roentgen Society at St. Martin's Town Hall, on November 5th, 1897. The number also contains five large skiagraphic plates and other interesting matter. A supplement entitled "Radiography in Marine Zoology," by R. Norris Wolfenden, M.D., is added. This supplement treats of the Echinodermata, and is illustrated with thirty-six excellent skiagraphs and photographs.

* *See* article, "Measurement of the Earth," KNOWLEDGE, June, 1897, p. 148.

Letters.

[The Editors do not hold themselves responsible for the opinions or statements of correspondents.]

"THE LIFE-HISTORIES OF THE BRITISH MARINE FOOD-FISHES."

To the Editors of KNOWLEDGE.

SIRS,—Kindly allow me a few words to conclude the correspondence on this subject. I have not denied, as your reviewer states, that "the work of St. Andrews is put more prominently forward than work done elsewhere," in our book. Such is, no doubt, the case; and it is, as he remarks, "not unnatural," considering that by far the greater proportion of British "fishery" work has been done there, or in direct connection therewith. This is a different matter from "ignoring" the work done elsewhere.

Your reviewer's statement that "Mr. Cunningham led the way" in the subject of the growth-rate of fishes has no foundation of truth. This worker published his first paper upon the subject in 1890, and at periods varying from five to twelve years prior to this the works of Dr. McIntosh, Captain Dannevig, and Dr. Meyer had appeared. Without further instance, your readers may be reminded that the two latter still stand as the best known authorities upon the growth-rate of the cod and herring respectively.

"The credit of the discovery of the hermaphroditism of *Myxine*" is not "given to Dr. Nansen." A passing reference to Dr. Nansen's work is mentioned in a *quotation* from another paper, in connection with which the reasons for its selection were given.

The life-history of *Myxine* did not fall within the scope of our work, or, of course, the labours of W. Müller, Cunningham, Weber, etc., would have been referred to.

With regard to *Nature*, my remark was to the effect that Dr. Lankester was allowed, under pretext of reviewing our work, to make certain false statements outside the pale of legitimate criticism, judged by the widest standard; and that the editor, in the opinion of a great many of his readers, showed a partiality in not allowing a contradiction.

Your reviewer considers my remarks "hardly in good taste" because Mr. Cunningham was similarly denied on a prior occasion. Surely this fact, which could not have been known except to Mr. Cunningham himself and his most intimate friends, merely corroborates my remark that your contemporary has been "not unknown" for such unfair treatment of authors. Mr. Cunningham has reason, judging from your reviewer's statement, to complain of his treatment, and still more to complain of the invidious position in which your reviewer has attempted to place his work.

In conclusion, I must now leave it with your readers to judge for themselves how far your reviewer has established his position that we have ignored the work of others in our labours.

The University, St. Andrews. A. T. MASTERMAN.

[In my notice of "British Marine Food Fishes," I remarked: "Between the marine biologists of the North and South there is something of a spirit of rivalry, the result being that each school is inclined to ignore, more or less, the work of the other—or, at any rate, not overburden it with praise." No impartial critic, familiar with the facts, could deny that every word of this sentence is true. It will be noticed that I did *not* assert that Dr. McIntosh and Mr. Masterman had "ignored the work of others in their labours": but surely the first paragraph of Mr. Masterman's letter justifies my position.

Mr. Masterman (p. 291) asked for an instance of "Mr. Cunningham's work which had not been alluded to and

freely acknowledged." I gave him the case of the work on the growth of fishes, referred to in a cursory manner which deprives it of any importance. Of course, Mr. Masterman may be permitted to have an opinion of his own as to what work is important, but marine biologists are also at liberty to challenge it.

With regard to the hermaphroditism of *Myxine*, the quotation is from a paper by Mr. Masterman himself, and the words used are: "We may cite Nansen's observation of the protandric hermaphrodite condition of *Myxine*." This certainly gives the idea that the hermaphroditism was discovered by Dr. Nansen.

As to *Nature*, Mr. Masterman distinctly ascribed "partiality" to the editor in the matter of the review of his book. He knows that Mr. Cunningham's work was treated in exactly the same way that the work of Dr. McIntosh and himself was treated, and yet he has not the good grace to withdraw his charge of partiality. I cannot say that Mr. Cunningham sent a reply to the criticism of his own work, but I know that no reply was published in *Nature*, any more than was Mr. Masterman's reply to Prof. Lankester's review. It is a pity that there are authors like Mr. Masterman ever ready to resent fair criticism and impugn editorial actions.—THE REVIEWER.]

TESTING MULTIPLICATION AND DIVISION.

To the Editors of KNOWLEDGE.

SIRS,—The properties of "the mystic number three," and its square *9*, referred to by your correspondents on page 292 of your magazine, bring one back to the earliest recollections of one's school days, when the multiplication sums were tested by "casting out the nines," as it was then called. The property that the sum of the digits of any integer, divided by *9*, gives the same remainder as if the number itself were divided by *9*, is a natural consequence of our decimal notation. Had the notation been duodecimal, *11* would have possessed the same property; and, even in the decimal notation, 11 may be used with almost equal ease, and is a safer test. Beginning with the units, add the alternate figures, and, carrying to the tens, add the other alternate figures; then add what is over to the units of the sum. If the number thus obtained be divided by 11, the remainder is the same as if the whole number were divided by 11. This can be easily shown from the obvious fact that every even number of nines is divisible by 11. These properties are well known, and hardly need illustration. They are mentioned as introductory to what follows.

Some years since, I had to do with the multiplication and division of very large numbers, consisting of sixty figures and upwards. Finding that neither 9 nor 11 was a sufficiently reliable test, I was led to seek for something safer. The numbers, for distinctness, were arranged in periods of five figures each, beginning of course at the right. My test was to be adapted to this arrangement, and I soon found that 11111, consequently 99999, is divisible by 41; therefore 99999 99999, etc. It follows that if the sum of the periods, taken as separate numbers and carrying what is over to the units' place, be divided by 41, the remainder is the same as if the whole number were divided by 41.

The division by 41 may be abridged thus:—

Let the sum of the periods, found as above, be	37529
Subtract the largest multiple of 11111 contained therein ...	33333

41)4196 remainder 14,

showing that, if the whole number were divided by 41, the remainder would be 14.

Years afterwards I was requested by the late Prof. Cayley to verify some results, involving also very large numbers, but arranged in periods of three figures each. To this also it was judged expedient to use a test specially adapted to the arrangement. I saw that 111, therefore 999, is divisible by 37; and consequently that if the sum of the periods of three figures, taken as above, be divided by 37, the remainder is the same as if the whole number were divided by 37.

Take, for example, any number at random, say—

45 286 507 613 941

The sum of the periods is	2 392
and, adding 2, the unit of the second period in the sum, to the first period, we obtain finally	394
Subtract the largest multiple of 111 therein	333
The number thus obtained	61

divided by 37, leaves the same remainder, 24, as if the whole number were divided by 37.

A. GRAHAM.

Cambridge Observatory,
14th December, 1897.

ARTIFICIAL SUNSPOTS.

To the Editors of KNOWLEDGE.

SIRS,—With regard to Mr. East's experiments and your remarks in the December Number of KNOWLEDGE, is it possible that in the sun's surface we have anything similar?—that is, are the rice grains really the only partial consolidated matter that we see?—all the interior of the sun being in a gaseous state, under such conditions of extreme heat and pressure as to make chemical combination and luminosity impossible. I should like to see some remarks in your journal on this head. It seems to me one of impossibility; the photosphere would then be the very first stages of a crust formation on the sun.

December 10th, 1897. THOS. J. HADDY.

RARE BIRDS.

To the Editors of KNOWLEDGE.

SIRS,—Your issue for December contains an account of the shooting of four rare birds. Most probably each of these rare birds had mates and would have continued the race if let alone, but the collector comes with his gun and endeavours to make the rare bird an extinct bird. Of course, his specimen would become more valuable if this species of bird became extinct in this country, while if he allowed the bird to escape he would have no specimen at all. But is this a sufficient reason for shooting a bird that is doing no harm and is not intended to be eaten, and whose only crime is that very few like it are to be found in this country? Our object should be to preserve—not to destroy—such rare specimens. This would be admitted if they were domestic animals. It is only when an animal is wild that he is shot because there are few like him. If the shooting goes on there will soon be none.

It is time that we had a society for the preservation of rare animals and birds—unless, of course, they are mischievous like the wolf, which has now died out in the British Islands.

Every zoologist will admit that utility is not the only thing to be looked to as regards the preservation or destruction of a race of animals; and, if there is no reason

for the extirpation of any peculiar species, why should we seek to extirpate them merely because they appear to be dying out of their own accord? I would rather preserve them as long as possible.

The dying out of a race of animals, when natural, may often indicate a gradual change of climate or other physical conditions, the history of which it will be desirable to trace hereafter. The arrival of a new race may afford similar indications to the student of science. But if we ruthlessly shoot down every member of a race that is dying out and every new arrival on our shores, landmarks of this description will be lost. The "footprints on the sands of time" wear out soon enough without intentional obliteration.

W. H. S. MONCK.

[The killing of rare birds has of late formed the subject of innumerable letters in the daily press. These letters are invariably written by persons not sufficiently acquainted with the details of the subject to form an accurate opinion as to whether the killing of any particular bird is to the advantage or disadvantage of the study of British birds. By this we mean that unless certain birds are killed ornithology will not advance. Glance, for instance, at the second part of Mr. Howard Saunders's manual (just published). There are at least six birds out of the twenty-four there described which would never have been known to have visited the British Islands had they not been shot.

With regard to the birds mentioned by Mr. Monck, these were all stragglers, and we can confidently say that none of them would ever have bred in Great Britain had they been allowed to live, and certainly three of them would never have been identified unless they had been shot. We do not wish our readers to infer from the foregoing remarks that we uphold the killing of every rare bird. Far from it. We consider it an act of ignorant greed to destroy in Great Britain a bird such as a Golden Eagle or Osprey, which were formerly fairly plentiful as breeding species, but have now become very rare. We would remind Mr. Monck that the Society for the Protection of Birds, which has often been referred to in KNOWLEDGE, has been established some years, and has done and is doing very good work in the prevention of that very ignorant destruction to which Mr. Monck so properly objects.—EDS.]

MOVEMENT IN SPACE.

To the Editors of KNOWLEDGE.

SIRS,—I saw it stated the other day that one of our astronomers had made a calculation that the rate of movement of our sun in space was twelve miles a second.

This idea of "movement in space" is to me incomprehensible. What we call "movement" is a relative state of matter, and can only be measured against something "at rest." For instance, we call an object fixed or stationary on the earth, when really it participates in the earth's motion; so it is quite possible that a fly on the woodwork of a railway carriage may consider itself "at rest" when it pauses in its walk, although the train is travelling at its usual speed.

As it would appear from our limited knowledge of the universe that a state of absolute rest is impossible, it would be interesting to know how this movement of the sun can be measured with anything approaching accuracy.

If you consider this a suitable subject to appear in your very interesting magazine, you would much oblige,

IGNORAMUS.

["Ignoramus" is quite right in supposing that motion in space can only be measured by taking some origin which we suppose fixed. In deducing the solar motion we assume that the group of stars which we employ for the purpose have, as a whole, no tendency to drift in any direction—or, in other words, that their centre of mean position is at rest. This centre of mean position is thus the fixed origin to which the solar motion is referred. The whole system of stars under discussion, including our sun, may have a common drift in some direction, but this we are unable to determine.]

Notices of Books.

With Nature and a Camera. By Richard Kearton, F.Z.S. Illustrated from Photographs by Cherry Kearton. (Cassell.) 21s. Perhaps we expected too much of Mr. Kearton, judging from reports which reached us before the publication of his book. However that may be, we are disappointed. There are many good things in the book, but it is our candid opinion that the author has been too hasty in putting his work before the public, for it bears unmistakable signs of "padding." A number of the photographs are not of sufficient interest for publication, while to others a great deal too much space has been given, making the book large, expensive, and annoying to the reader. The most glaring examples of "padding" are two full-page illustrations of a rabbit burrow closed and a rabbit burrow open (pages 178 and 179), a common enough sight to everyone. If the photographs had been "pictures" we should, perhaps, have excused the author, since his book is mainly a "picture book"; but they are by no means pictures, and are made additionally hideous by a large bottle in the foreground. The letterpress also is by no means free from "padding." A number of the facts—some of them here set down as extraordinary—have been published scores of times before. It is well known that the song thrush sings occasionally on fine nights; yet the author, who has had some experience, was "astonished to hear a thrush commence to sing" one moonlight night, and considers that in this fact he has "unmistakable

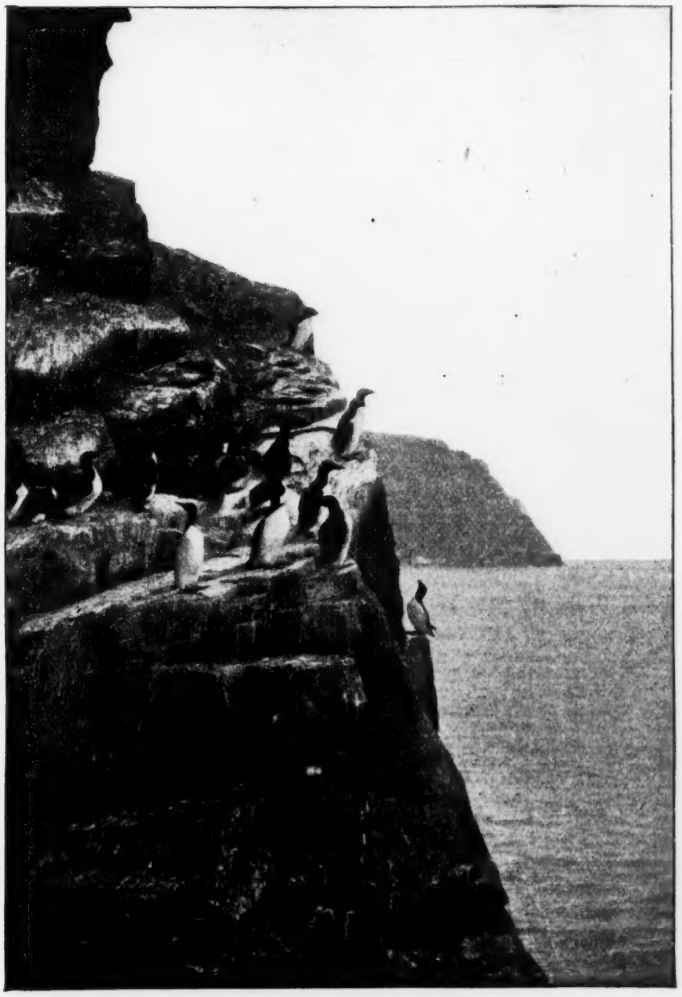
Lesser Black-backed Gulls. (From "With Nature and a Camera.")

proof" that "birds may, upon occasion, mistake the rising of the moon for the coming of another day." The author gives a detailed account of an old shooter and his favourite "setter" bitch, and on page 161 he gives a

photograph of the two; but the "setter" is an unmistakable pointer. Having said so much of what we consider to be bad judgment and error, we gladly pass on to the good points in the book. These are chiefly to be found in the photographs, a great number of which are exceedingly fine. We would especially draw attention to the following:—Barn owl, photographed by flashlight (page 243), kingfisher (page 357), cormorants and guillemots (page 251), common gull's nest (page 269); and to those which we have been able, by courtesy of the publisher, to here reproduce. Mr. Cherry Kearton, who has taken the photographs for his brother's book, has had many perilous adventures, as all who climb cliffs—and especially those who carry a camera with them—must; but we cannot help thinking that Mr. Kearton has often run into unnecessary dangers. Many of the things he has photographed in difficult places could have been found in more accessible situations. We recommend the book with the qualification that if the author has not made "much ado about nothing," he has certainly made too much of not a very great deal.

Ornamental Design for Woven Fabrics. By C. Stephenson and F. Suddards. (Methuen.) Illustrated. 7s. 6d. We are not by any means convinced that the authors of this handsome-looking book have succeeded in their laudable desire to "bring the necessary knowledge within a narrower focus, and thereby make it more easily accessible." In attempts like this to find the path of least resistance to a useful knowledge of a science or an art, there is always a danger of retarding progress by, in a manner, increasing friction—making the pathway too constricted for one to get through with comfort. For example, although a knowledge of elementary geometry is assumed, a single plate is given showing the construction of the most simple, and at the same time useful, figures, and then in a few pages their application to the design of woven fabrics is dispensed with. The authors, indeed, exhibit a clear insight as to the way in which such figures are utilized in the designer's office; but a beginner would be all at sea in practice if only equipped with such knowledge as is here so over condensed as to be nearly, if not quite, indigestible. Coming to the main part of the work, however, we find a different state of affairs. Dealing with the laws of composition, plant forms in textile designs, limitations imposed, drop-pattern, repeats, and so on, as well as in the arrangement of the warp-threads and their sequence in rising and falling in order to attain any given pattern, all is clear, and in the highest degree commendable. The book is in every way handsome, and the illustrations are of first-rate quality.

The Rise of Democracy. By J. Holland Rose, M.A. The Victorian Era Series. (Blackie & Son.) 2s. 6d. This is a wholly inadequate treatment of a great subject, due in a large measure to the laudable desire of the author to compress a vast amount of historical detail into a small compass. The result is a more or less disjointed catalogue of names and incidents, often incomplete, and always lifeless. Admittedly circumscribed in the space at his disposal, Mr. Rose has persistently stood in his own way, and filled valuable space by recounting his own interpretation of the facts, so that his reader is often unable to appreciate the picture by reason of the obtrusive nature of the showman. However interesting the author's opinions may be to Mr. Rose, he should remember they are of no value to his reader. Some haste, too, is apparent in the text, where we find Sir Francis Burdett, the famous member for Westminster, figures as Sir Thomas Burdett, and Richard Carlile as Carlisle; while the alleged "toning down" of John Stuart Mill is, of course, an entire misapprehension of the facts. Then we do not like to find such phrases as the "immense vogue" of Darwin, or the "viewy schemes" of Owen, in a book which promised in the preface to be "scholarly." The index, too, is hasty, incomplete, and lacking in method. Yet, notwithstanding these defects, Mr. Rose's little book will be found to be an interesting sketch of the growth and expansion of representative institutions in England, as well as of the patient doggedness and prescience of our countrymen. But it leaves the task yet unfulfilled of writing the history of the rise of democracy.



Guillemots on Cliff. (From "With Nature and a Camera.")

An Introduction to Geology. By Wm. B. Scott. (Macmillan.) Illustrated. 8s. net. New strata of books, so to speak, are being continually superposed on pre-existing books of the same kind, and it too frequently happens that they contain no fossils, as it were, to invest them with special characteristics sufficient to differentiate them from their predecessors. In a sense, Prof. Scott's work is of this kind—that is to say, the book is not of any particular value to English students, but rather a class-book for American students of geology. The principles of the science are elucidated in a manner closely corresponding with our own standard works on the same subject. To those, however, who have advanced beyond the confines of an acquaintance with first principles, and are prepared for fresh fields and pastures new, we may say there is here much that is worthy of careful study—matter to fill many a gap, confirm or accentuate doubtful points, and, above all, a panorama of familiar phenomena in a new and attractive dress, which will lend a more extensive, more diversified, and more persuasive view to the mental eye.

John Hunter: Man of Science and Surgeon. By Stephen Paget. (Unwin.) Portrait. 3s. 6d. Among the greatest men that England has produced must be reckoned those who have built up the science of medicine in its broadest sense, and among savants of this kind Hunter was head and shoulders above his contemporaries—one of the master builders of the Temple of Hygiea. The whole secret of his extraordinary achievements in life can be expressed in a few words: "Don't think—try; be patient—be accurate." A great deal, it is true, may be learnt by thinking; but when experimental facts are brought to bear upon a certain theory, more exact conclusions can be deduced than by mere speculative opinion without the foundation stones of exact observation. As a boy, Hunter was an observer of nature, and did not care much for his school books; and when he came to London to work with his brother William, he studied hard for three years, spending his time mainly in the dissecting rooms night and day. Thus far, Darwin and Hunter, in so many ways alike, went both of them along the same high road; here the road divides at a narrow angle. Hunter went forward from human anatomy to all anatomy and physiology, and from these to medicine and surgery, and from all of them together to a profound study of life, alike in health and disease, in all structures, at all stages. To the medical student of the present day the correspondence here given between Hunter and his famous pupil, Jenner, must present a strange picture. Our museums now supply all the requisites for study, but in Hunter's time every student had to cater for himself: find specimens for dissection where he could; get his chemical knowledge from one source, anatomy from another, and so on; all outside the hospitals, which were not organized for complete instruction. The book is one of a series—"Masters of Medicine"—and will include among others: Harvey, Jenner, Simpson, Helmholtz, Stokes, Bernard, Brodie, and Sydenham. Provided subsequent volumes are of equal merit with this one, the series will form a most delightful record of the development of the healing art.

Recent and Coming Eclipses. By Sir Norman Lockyer. (Macmillan.) Illustrated. 6s. net. By this time Sir Norman Lockyer may be regarded as a veteran eclipser. During the last quarter of a century he has captained many expeditions, and anything he has to say on eclipses will be sure to command the attention of all interested in such phenomena. In describing what he saw in 1871 the author gives us some idea of the imposing grandeur of an eclipse in these words: "There, in the leaden-coloured, utterly cloudless sky, shone out the eclipsed sun—a worthy sight for gods and men. There, rigid in the heavens, was what struck everybody as a decoration—one that emperors might fight for—a thousand times more brilliant even than the Star of India, where we then were; a picture of surpassing loveliness, and giving one the idea of serenity among all that was going on below; shining with a sheen of silver essence; built up of rays almost symmetrically arranged round a bright ring above and below, with a marked absence of them right and left, the rays being composed of sharp radial lines, separated by furrows of markedly less brilliancy." Although the author, according to the title page, purports to give in his book notes on the eclipses of 1893, 1896, and 1898, considerable space is taken up with the subject of eclipses generally. Seeing that the sun itself is essentially a star, we quite expected to find ample reference to stellar researches—a sort of discussion on the comparative anatomy of suns—but we are of opinion that such allusions as that found on page 105 are quite out of place in a popular book. Sir Norman says: "I am glad to see that Sir William Huggins, who appears to be ignorant of my quarter-of-a-century-old work, has quite recently arrived independently at the same conclusion." The arm-chair astronomer doesn't want condiment of that sort. As regards the great diversity of work to be carried on during the precious moments of totality, we have in this handy book an admirable description such as could only emanate from one thoroughly conversant with every aspect of the phenomenon. Difficult as the subject is, we can readily follow the master through every labyrinth. The heterogeneous mass of facts gleaned by a multitude of observers in all parts of the world during eclipses spread over half a century, are here put through the intellectual mill and worked into a shape which one can appreciate. We see how the sun and stars are, as it were, comparable to the several orders of animals, all more or less alike, and differing from one another only in detail. The large section dealing with the coming eclipse, however, appears to us foreign to the general reader, and fitted only to the wants of the few who actually take part in eclipse work. It bears a closer resemblance to printed instructions from a leader to his followers than literature on a popular subject for the million. The illustrations are of very unequal merit.

Electricity in the Service of Man. By R. Wormell, D.SC., M.A. Revised and enlarged by Mullineux Walmsley, D.SC. (Cassell.) Illustrated. 7s. 6d. Perhaps the best evidence of the worth of this work is its continued issue, time after time, in improved form. Evidently no expense has been spared in making the book a thoroughly reliable exposition, in popular phraseology, of the principles which underlie all the practical applications of electricity in every-day life. The publishers, in this case, certainly give a maximum of value for a minimum of outlay—a circumstance which will be sufficiently apparent when we point out that there are a thousand printed pages and as many illustrations.

The Method of Darwin. By Frank Cramer. (Chicago: McClurg & Co.) This book is an analysis of the scientific method of Charles Darwin. Darwin's works have been chosen as a basis on account of—"(1) the desire to confine the discussion to the writings of a single author; (2) the fact that his works cover a wide range of subjects; and (3), above all, the fact that Darwin's investigations, and the reasoning based upon them, have furnished the biological sciences with their dominant principles"—and also because "Darwin's custom of presenting all sides of a case very frequently led him to expose the original course of his thought and the order of his discoveries." The author has chosen an excellent and certainly a neglected subject.

In Darwin's works he has the best foundation possible for a study of scientific method, and above all he has planned his book well and written it lucidly.

After a brief explanation of logical processes we have the following chapters, each one being discussed in connection with well-chosen examples from Darwin's works:—Darwin's Views of Method, Starting Points, Exhaustiveness, Negative Evidence, Classification, Analogy, Induction, Deduction, Unverified Deductions, Erroneous Deductions, General Discussions, Logical History of the Principle of Natural Selection, and Conclusion.

We have given an idea of the scope of the book and heartily recommend it, especially to those who are starting out on scientific work of whatever kind. Our only complaint is that the book is not larger and more exhaustive.

BOOKS RECEIVED.

Bureau of American Ethnology—Sixteenth Annual Report. (Government Printing Office, Washington.)

The Sun's Place in Nature. By Sir Norman Lockyer. (Macmillan.) Illustrated. 12s.

By Roadside and River—Gleanings from Nature's Fields. By H. Mead Briggs. (Elliot Stock.) Frontispiece. 3s. 6d.

The Journals of Walter White, formerly Assistant Secretary of the Royal Society. With Preface by William White. (Chapman & Hall.) Portrait. 6s.

Observational Astronomy. New Edition. By Arthur Mee. (*Western Mail*, Limited, Cardiff.) Illustrated. 2s. 9d. post free.

Modern Architecture. By Heathcote Statham. (Chapman & Hall.) Illustrated.

The Encyclopædia of Sport. Edited by the Earl of Suffolk and Berkshire, Hedley Peek, and F. G. Aflalo. Vol. I. (Lawrence & Buller.) Illustrated. 25s.

Obituary.

WE regret to record the death of Dr. F. A. T. Winnecke, at Bonn on the 3rd December, 1897, in the sixty-third year of his age. Curiously enough, the comet which bears his name, and having a period of 5·818 years, is expected to return to perihelion almost at any time in the early part of the present year. He was born in Hanover on 5th February, 1835, and received his education at Berlin. After assisting Encke (Encke's comet, period 3·303 years, is also expected about May of this year) at the observatory there, and afterwards Argelander at Bonn, he accepted an appointment in Russia, and many years of his greatest scientific activity were spent at Pulkowa. In 1868 Dr. Winnecke took charge of the observatory at Carlsruhe, and in 1872 he was nominated Professor of Astronomy at the newly founded University of Strasburg. He was elected an Associate of the Royal Astronomical Society in 1863. Cometary astronomy always had for him great attractions; besides the periodic comet which bears his name he found several others, receiving the prize of the Vienna Academy of Sciences for his cometary discoveries.

BOTANICAL STUDIES.—I.

VAUCHERIA.

By A. VAUGHAN JENNINGS, F.L.S., F.G.S.

THE study of plants has till recent years occupied a somewhat different position from that of its sister sciences. When geology was rousing the interest of the intellectual world by its conclusions as to the history of the earth, and while zoology formed, mainly, the battle-ground of the evolutionists and their adversaries, botany still remained a science of the collector and the classifier. Only comparatively lately has it been able to take its place as a philosophic science on a level with zoology. Its acquirement of this position has been due to the increasing number of capable students, and the improvement of microscopic methods of research. It is possible that a recognition of the importance of microscopic study has sometimes led botanical teachers too far in contrasting their work with that of the earlier students of the field and the herbarium. There may be room for a protest against the predominance of micro-technicality, but the work of the microscope in giving botany its proper position in the life sciences can never be seriously exaggerated.

The discovery of the life histories of lower plants, of the details of the reproductive processes in higher cryptogams, and the demonstration of the relationship between them and those of the flowering plants, form one of the most striking chapters in the history of biological research.

Though these results have been arrived at only by long labour, by the employment of high magnification and refined methods of preparation, it is yet by no means impossible for the amateur microscopist to see for himself a great number of the more important phenomena in question in this line of investigation. It is proposed to call attention to a few important types, which form, as it were, landmarks in the world of plants.

As a starting point we may select a common and easily obtainable plant in which the reproductive processes are simple and readily observed. The species of the genus *Vaucheria* form green velvet-like patches on damp ground or thick felted masses of threads in ponds and ditches. With a low-power pocket lens only, the branched and interlacing threads can be distinctly seen, and it may be observed that some carry small rounded excrescences on the side, while others may be darker in colour and enlarged at the tip.* If a specimen is mounted in water and examined with a low power of the microscope, it will be found that the whole plant consists of a cylindrical tube of protoplasm enclosed by a delicate cell wall;† but there are no transverse walls crossing the tubes. If the green colouring matter, or chlorophyll, is dissolved out by soaking in alcohol, and the specimen treated with iodine solution, or other suitable stain, it will be found that the protoplasm contains numerous small specialized portions or nuclei which are deeply coloured.‡

The plant is thus a protoplasmic body with numerous nuclei, but the division of these nuclei is not followed by formation of new cell walls, and the plant remains unicellular.§ There is a wrong impression produced if we speak of the higher plants as aggregations of cells, as if they were so many bricks; and the group of algæ to which *Vaucheria* belongs is of special value in reminding us of the fact that the cell walls are of secondary importance in comparison with the protoplasm and nuclei. It is the great series of the *Siphonaceæ* which includes a large number of marine seaweeds often of considerable size and complex structure. To it belong such varied types as the green furry *Codium*, common on the piles of our sea-coast piers; the feathery *Bryopsis* of our rock pools; the polymorphic *Caulerpa* and the calcareous coralline-like *Halimeda* of warmer climes; and the quaint little umbrella-like *Acetabularia* of the Mediterranean. Such variety of form and wide distribution suggest a great antiquity for the group, and there is little doubt that in the Eocene *Dactylopora* and *Ovalites*, and the Triassic *Gyroporella*, we have

* *Vaucheria* plants are often sterile; and the enlargement of the ends should be looked for after the plant has been some time in darkness.

† By adding a weak (two per cent.) solution of common salt the protoplasm will contract away from the wall owing to the abstraction of water. ("Plasmolysis.")

‡ It is not always easy to demonstrate them by such simple staining, and special methods may have to be employed.

§ The term "cœnocyte" for such large multinucleate cells is a convenient one, and coming into general use.

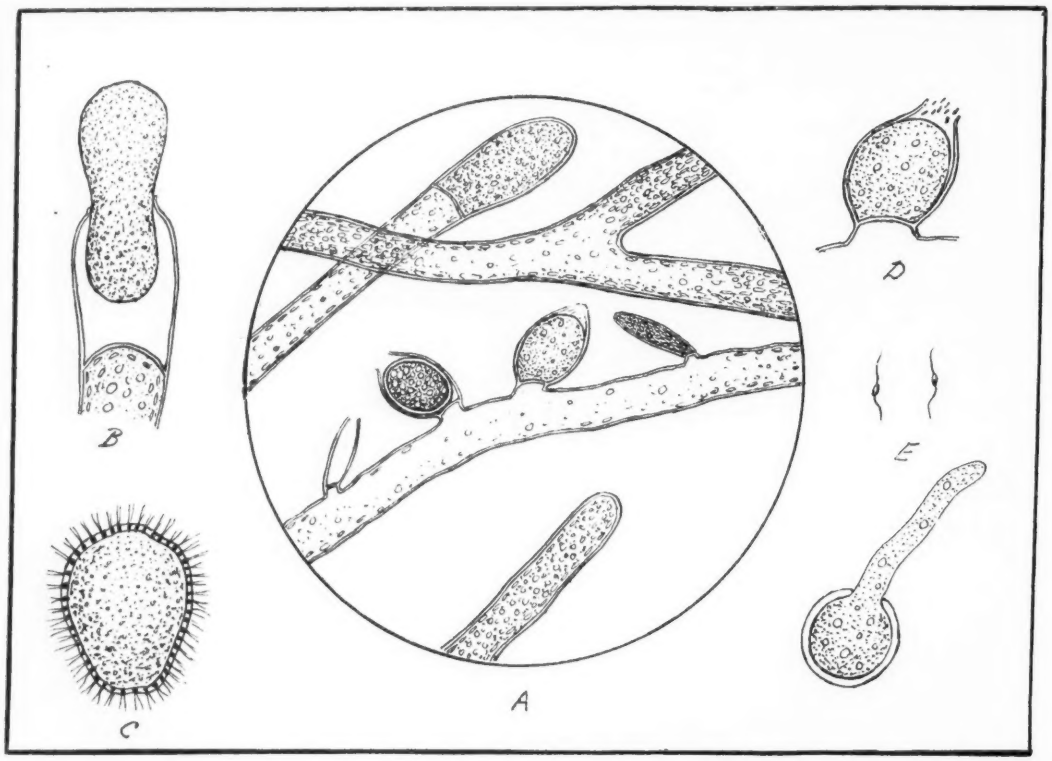
direct evidence of its geological age. These questions are outside our present object, but indicate how far the green weed from the garden path might lead us.

The special feature we want to observe is the mode of reproduction of the plant, and it will be found that it propagates itself by two distinct methods.*

In the first case there is an aggregation of the protoplasm at the ends of certain threads, and in time this specialized portion makes its way through the terminal wall and swims about by means of vibrating cilia, which occur in pairs all over its surface. In time this liberated mass of protoplasm loses its cilia, settles down, develops a cellulose wall, and passes into a resting stage. Later on, it germinates and grows directly into a new

the main axis. Their contents are, however, cut off from the latter by a transverse wall or septum. The larger inflated bodies contain each a rounded protoplasm mass which is the oosphere or egg-cell. The narrower tubular structures are the antheridia, and at the right stage will be found full of minute antherozoids formed by repeated subdivision of the protoplasm and nuclei. These antherozoids or spermatozoids are minute oval bodies each with a pair of cilia, by means of which they move.

They escape from an aperture at the apex of the antheridium, which in most species curves round so as to approach the top of the oogonium.* The wall of the latter becomes gelatinous at this point, and the antherozoids pass through and effect the fertilization of the oosphere.



A.—*Vaucheria aversa*.—The filament in the centre shows two Oogonia and two Antheridia. The Antheridium on the left is empty, and the fertilized Oosphere in the corresponding Oogonium has developed a thick wall. In the upper filament the protoplasm is aggregated at the apex, and shut off by a septum prior to the formation of a Zoogonidium. B.—The Cœnocytic Zoogonidium of *Vaucheria* passing out from the apex of a filament. C.—The Cœnocytic Zoogonidium of *Vaucheria*, showing numerous peripheral nuclei, with pairs of Cilia opposite each. D.—An Oogonium, with Antherozoids passing through the mucilaginous apical area. E.—Antherozoids (or Spermatozoids). F.—Germination of an Oospore or Oosperm.

Vaucheria plant. This process of renovation of physiological energy in a special part of the protoplasm is termed "rejuvenescence."

For the other and more important method of reproduction, one must examine the small protuberances which occur here and there on the sides of the threads. These will be found to be tubular or oval outgrowths from the filament enclosed by a cell wall continuous with that of

Subsequently the oosphere surrounds itself with a thick protective wall, passes through a period of quiescence, and in time germinates, growing at once into a new plant.

Such is a brief summary of the life history of this common but no less interesting plant.

The type has been selected as affording a simple example of oogamous reproduction; and the important

* It should be noted the type of oogamous reproduction here described occurs in *Vaucheria* only. In the other genera the process of reproduction is in some cases still unobserved; in others it takes place by conjugation of similar, or slightly dissimilar, free swimming "gametes."

* The number and distribution of the oogonia and the form of the antheridia differ in the various species. The one chosen for the illustration is a fresh-water species, and was collected in a pond near Croydon. The commoner *V. sessilis*, on damp earth, has the curved antheridium; as also *V. hamata*, *V. racemosa*, and others. The type here shown is the simplest of all, and has not been figured in the usual text-books.

points to note in connection with our present purpose are, firstly, that the "fruit" is only the fertilized oosphere without any accessory or surrounding growths; and, secondly, that when this "oospore" germinates it produces a new plant like that on which it grew.

THE FACE OF THE SKY FOR JANUARY.

By HERBERT SADLER, F.R.A.S.

A FEW small spots may still be occasionally detected on the solar surface.

Conveniently observable minima of Algol occur at 11h. 49m. P.M. on the **16th**, at 8h. 38m. P.M. on the **19th**, and at 5h. 27m. P.M. on the **22nd**.

Mercury is in inferior conjunction with the Sun on the **6th**. During the last third of the month he is visible as a morning star, but under very unfavourable conditions in these latitudes, owing to his great southern declination. On the **21st** he rises at 6h. 23m. A.M., or about one hour and a half before the Sun, with a southern declination at noon of 20° 51′, and an apparent diameter of $7\frac{3}{4}''$. On the **31st** he rises at 6h. 25m. A.M., or about one hour and a quarter before the Sun, with a southern declination of 21° 47′, and an apparent diameter of $6\frac{1}{2}''$. He is at his greatest western elongation (25°) on the **29th**. While visible he describes a direct path in Sagittarius without approaching any conspicuous star.

Venus is too near the Sun to be observed, as is also the case with Mars.

Ceres is still in an excellent position for observation. She souths on the **1st** at 11h. 35m. P.M., with a northern declination of 28° 4′, her stellar magnitude being about $7\frac{1}{4}''$. On the **10th** she souths at 10h. 40m. P.M., with a northern declination of 28° 37′. On the **20th** she souths at 9h. 51m. P.M., with a northern declination of 29° 5′. On the **31st** she souths at 9h. 7m. P.M., with a northern declination of 29° 27′, her stellar magnitude being about $7\frac{1}{2}''$. During the month she describes a retrograde path in Auriga.

Jupiter is now beginning to be fairly well placed, as regards his times of rising, for the amateur. On the **1st** he rises at two minutes before midnight, with a southern declination at noon of 2° 32′, and an apparent equatorial diameter of 39″. On the **11th** he rises at 11h. 22m. P.M., with a southern declination of 2° 43′, and an apparent equatorial diameter of 40″. On the **21st** he rises at 10h. 44m. P.M., with a southern declination of 2° 47′, and an apparent equatorial diameter of $40\frac{1}{4}''$. On the **31st** he rises at 10h. 4m. P.M., with a southern declination of 2° 43′, and an apparent equatorial diameter of 41″. During the greater part of the month he describes a very short direct path in Virgo, without approaching any conspicuous star. He is stationary on the **25th**.

Both Saturn and Uranus do not rise till long after midnight during the month, and they are both very badly placed for observation in these latitudes.

Neptune is very well situated for observation, rising on the **1st** at 2h. 28m. P.M., with a northern declination of 21° 44′, and an apparent diameter of $2\frac{1}{2}''$. On the **11th** he rises at 1h. 48m. P.M., with a northern declination of 21° 43′. On the **21st** he rises at 1h. 2m. P.M., with a northern declination of 21° 42′. On the **31st** he souths at 8h. 32m. P.M., with a northern declination of 21° 42′. During the month he describes a short retrograde path in Taurus, in a region barren of naked-eye stars.

January is a favourable month for shooting stars, the most noted shower being that of the *Quadrantids*, the radiant point being in R.A. 19h. 12m. and 53° north declination, the greatest display being visible during the morning hours of January **1st** to **3rd**.

The Moon is full at 0h. 24m. A.M. **on the 8th**; enters her last quarter **at** 3h. 44m. P.M. on the **15th**; is new at 7h. 25m. A.M. **on the 22nd**; and enters her first quarter at 2h. 33m. P.M. on the **29th**. Many of the larger stars of the Pleiades will be occulted on the evening of the **3rd**.

There will be a partial eclipse of the Moon on the evening of the **7th** and early morning of the **8th**. The first contact with the penumbra takes place at 9h. 11m. on the **7th**; the first contact with the shadow at 10h. 57m. P.M., at an angle of 169° from the Moon's limb towards the east (viewed for direct image). The middle of the eclipse will occur at 11h. 45m. P.M., about $\frac{1.6}{100}$ths of the disc being obscured. The last contact with the shadow takes place at thirty-two minutes after midnight on the **7th**, at an angle of 143° from the north point of the Moon's limb towards the west. The last contact with the penumbra occurs at 2h. 18m. A.M. on the **8th**. There will be a total eclipse of the Sun on the morning of the **22nd**, but it will be invisible in the British Islands.

Chess Column.

By C. D. LOCOCK, B.A.

Communications for this column should be addressed to C. D. LOCOCK, Burwash, Sussex, and posted on or before the 10th of each month.

Solutions of December Problems.

(By W. J. Ashdown.)

No. 1.

1. Q to R4, and mates next move.

No. 2.

1. R to B2, and mates next move.

CORRECT SOLUTIONS of both problems received from Alpha, J. T. Blakemore, J. M'Robert, W. de P. Crousaz, J. E. Gore, G. Coules, E. C. Noton.

Of No. 1 only, from H. H. Thomas, A. H. Doubleday, Capt. Forde, W. Clugston, G. M. Norman.

Of No. 2 only, from G. G. Beazley.

No less than four solvers gave 1. R to B3 for No. 2, overlooking the reply 1. . . . B to K8. The correct key, it will be observed, prevents the dual after 1. . . . B×P.

H. H. Thomas.—In No. 2, if 1. B to Kt3, Kt to B5 (!), and there is no mate. It is a magnificent "try."

G. G. Beazley.—If B×P, Black retaliates (ch).

A. E. Whitehouse.—In No. 2, B×Kt is met by the Queen moving on to the Rook's file. B×KtP in No. 1 loses a piece.

H. S. Brandreth.—You will have seen that your solution of Mr. Challenger's three-mover was correct; not so that of Mr. Slater's insidious two-mover.

W. Clugston.—Thanks for the problems, which shall be examined, and if, as we expect, found worthy, receive early publication.

G. Coules.—Thanks for the three-mover. The only obvious drawback consists in the two "short mates" after two of the King's moves, which look as if they should lead to main variations, and lead, therefore, instead, to disappointment. The problem, we think, could be improved by abolishing the two Rooks, and, if possible, utilizing the KB more.

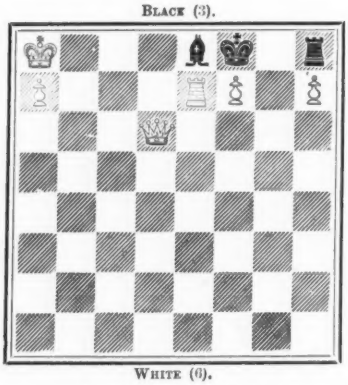
XUM

PUZZLES.

By C. D. Locock.

No. 1.

BLACK (3).

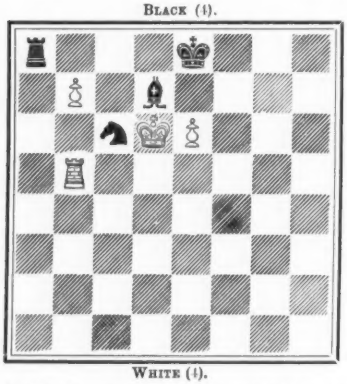


WHITE (6).

White compels Black to mate in two moves.

No. 2.

BLACK (4).

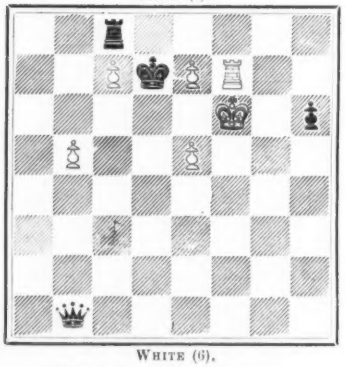


WHITE (4).

White, with Black's assistance, is mated in two moves. (The Black King has not moved).

No. 3.

BLACK (4).



WHITE (6).

White to play and draw.

[The solution of these positions requires what is known as a "liberal interpretation" of the laws of chess, particularly that relating to Pawn promotions. They are not serious studies, but possibly not devoid of amusement.]

CHESS INTELLIGENCE.

M. Janowski defeated Herr Walbrodt in their match at Berlin by five games to three, a very creditable performance considering that the score at one time was three to one in favour of Herr Walbrodt, who had only to draw one of the next two games in order to win the match. When the score reached three all, the match was prolonged for another three games according to the conditions arranged, and M. Janowski winning the first two of these became the victor.

The Amateur Championship Meeting will be held this year at Belfast. The experiment is a novelty, and the distance from London may militate against a very representative entry. The Irish amateurs, however, will have a good opportunity of testing their strength.

Under the title of "Pollock Memories," a selection of the games of the late W. H. K. Pollock will shortly be issued. A biography and portrait will be included, and the games will be annotated. The price to subscribers will be two shillings and nine pence post free. Address: Mrs. F. F. Rowland, 6, Rus-in-Urbe, Kingstown, Ireland.

We regret to announce the death of the Rev. E. J. Huntsman, president of the Sheffield Chess Association, and formerly a well-known figure at the meetings of the Counties' Chess Association.

It is stated that Mr. Lasker, who has abandoned chess lately in favour of science, will return to England in the summer and renew his former pursuit.

In the Championship Tournament of the City of London Chess Club the best scores so far have been obtained by Dr. Smith, Mr. H. W. Trenchard, and Mr. W. Ward.

A four-handed chess match, played on December 13th between the British Chess Club and the Four-handed Chess Club, resulted in a draw, each side scoring one game.

KNOWLEDGE, PUBLISHED MONTHLY.

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PLATE.—Artificial Sunspots.

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